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V.38

The Australasian Journal
of Philosophy

VOL. 38 MAY, 1960 No. 1

# CLUSTERS

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### 1. *Sets and Classes*

Words such as 'set', 'class' and 'aggregate' are ambiguous. One normally refers to an aggregate by means of a definition, and the aggregate so referred to has certain members (sometimes none). The ambiguity hinges on the question: When are two references to an aggregate to count as references to 'the same' aggregate? One possible answer is: When the members of the aggregate referred to on one occasion are the same as those of the aggregate referred to on the other occasion. To adopt this criterion is to use 'aggregate' in the extensional sense; I shall reserve the word 'set' for this sense. Another possible answer is: When the definitions used on the two occasions are the same. To adopt this criterion is to use 'aggregate' in the intensional sense, for which I shall reserve the word 'class'.

It follows from the criterion of identity of a set that its membership can never change. It is self-contradictory to say that a set, S, will change in membership; for that is to imply that S nowadays has such and such members, but that later on 'the same set' (i.e. the set which then has the same members) will not have the same members. Hence a tenseless mode of speech is appropriate in talking of sets. For it makes no sense to say that something 'became' or that it 'will become' a member of a set, or that one set 'became' or 'will become' a sub-set of another. Sets are timeless entities, timelessly interrelated.

It follows from the criterion of identity of a class, on the other hand, that its membership can, in certain cases, change. Suppose that in 1940 I speak of 'the English peers', referring to those who are then peers; and that I again in 1950 speak of 'the English peers', referring to those who are peers in 1950. On both occasions I refer to those who, at the moment of speaking, fall under the definition 'English peers'. Since the definition is the same on the two occasions I am, on both occasions, speaking of the same class. But I do not refer to the same set of men on the

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two occasions, for in the meanwhile some peers will have died and been succeeded by their eldest sons. The class *English peers* had one set of members in 1940, and a different set of members in 1950.[1]

Since classes can change in membership one needs to use verbs with tenses in speaking of them. For something can come to be, or cease to be, a member of a class; one class can become or cease being a sub-class of another; two classes can come to have the same membership or cease to coincide in membership. On a certain day, for instance, Bertrand Russell became a member of the class *English peers,* a class that used to coincide with the class *English large-scale landowners,* and which will soon include, as a sub-class, the class *British ex-prime-ministers.*

## 2. *Referent Classes*

Classes are commonly defined by an attribute or set of attributes—for instance, the class *human females.* But I shall be concerned with that type of class—I call it a 'referent class'—which is defined by a relation and a term. It consists of all those things that have the defining relation to what I shall call the defining 'focus'. Thus the relation *friend of* and the focus *Smith* together define the referent-class *friends of Smith*; the relation *child of* and the focus *George and Margaret* define the referent-class *children of George and Margaret.* (I shall also count as a referent-class one defined by a relation, a focus and an attribute—for instance the class *daughters of George and Margaret,* defined by the relation *child of,* the focus *George and Margaret* and the attribute *female.*) Other examples of referent-classes are: *objects equal in length to the standard metre*; *theorems derivable from the axioms of Principia Mathematica.* There are great differences between such classes, arising from differences in logical type between their defining relations. In the first part of this paper I distinguish the main types of referent-class, corresponding to the main types of dyadic relation.

## 3. *Non-Transitive Relations and Circles*

The relation *taller than* is transitive, for if A is taller than B and B taller than C, it follows that A is taller than C. But

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[1] If one wishes to talk about sets it is often inconvenient to make use of expressions which, like 'English peers', designate different sets at different times. Among mathematical logicians, therefore, it is customary to avoid definitions like these, and to use only extensionally invariant definitions such as 'even numbers' or 'English peers at noon 1/1/1950'. My purpose, on the other hand, is to discuss classes, and, in particular, classes with variable membership.

*adjacent to* and *friend of* are both non-transitive relations. When A is adjacent to B and B to C, it does not follow that A is adjacent to C—nor is your friend's friend necessarily a friend of yours. Other non-transitive relations are: *two miles from, child of, employee of, citizen of, immediate consequence of.* A referent-class defined by such a relation will be called a 'circle'. Typical examples are the class *points exactly two miles from the point X* (which is also a 'circle' in the geometer's sense), and the class *friends of Smith* (Smith's 'circle' of friends). Other circles are: *the children of George and Margaret, the citizens of France, Woolworth's employees.*

### 4. *Fellow-Relations*

When two things or persons are fellow-members of the same circle they stand to each other in a derivative relation, which can be defined in terms of the circle's defining relation. Thus two members of the circle of Woolworth's employees stand to each other in the derivative relation of *fellow-employee* or *workmate*; two members of the circle of the citizens of France are *fellow-citizens* or *compatriots* of each other; two of George and Margaret's children are *siblings* of each other—'fellow children', one might have said. Derivative relations of this kind will be called 'fellow-relations'. They are all of them symmetrical: if A is a workmate of B, for instance, then B is a workmate of A.

Some fellow-relations are non-transitive, and some are transitive. This depends on whether the circle's defining relation is a many-many or a many-one relation. The relation *employee of,* for instance, is many-many. One man can have many employees, and one man can work for more than one employer. Therefore the derived fellow-relation *workmate of* is non-transitive. If A is a workmate of B, and B a workmate of C, it does not follow that A is a workmate of C. For A and B might both be Woolworth's salesmen by day, and B and C both be night-watchmen with T.A.A. The relation *citizen of,* on the other hand, if we assume that no person can be a citizen of more than one state, is a many-one relation. On that assumption the fellow-relation *compatriot of* is transitive. Any compatriot of a compatriot is also a compatriot. *Sibling of* is obviously also transitive.

### 5. *Simple Transitive Relations and Chain-Relations*

I mention transitive fellow-relations, however, only to get them out of the way, and to prevent confusion. My chief concern is with two other types of transitive relation. First of all there are ‘simple transitive’ relations, like *taller than, heavier than, earlier than, equal in length to, simultaneous with, invariably accompanies.* Secondly there are what I shall call ‘serial-’ or ‘chain-relations’. Two or more terms, taken in a certain order, constitute a series with respect to a non-transitive relation, if that relation holds between each term and the next. Thus Holland, Belgium, France and Spain, in that order, constitute a series with respect to *adjacent to*—each is adjacent to the next in the list. Sweden and Norway constitute a two-termed series with respect to the same relation. I shall say that the first member of any series is ‘serially related’ to the last. Holland, for instance, though not adjacent to Spain, is ‘serially adjacent’ to it; and Sweden, which is adjacent to Norway, is also, in my sense, ‘serially adjacent’ to Norway. Likewise A is ‘serially child of’ B if A is either a child of B, or a grandchild of B, or a greatgrandchild or a yet remoter descendant of B. A ‘serial child of B’ is, in fact, a ‘descendant of B’, in that sense in which one’s children are counted among one’s descendants. And the jargon term ‘serially a parent of’ means, in ordinary English, ‘ancestor of’, in that sense of ‘ancestor’ in which one’s parents are reckoned among one’s ancestors. I apologise for the jargon. But for each non-transitive relation a corresponding serial relation can be defined, and it is very convenient to have a uniform and short way of expressing it. Serial relations are, of course, transitive. Another example is *formally derivable from.* In formal systems the relation *immediate consequence of* is first defined—in terms, say, of variable substitution and the *modus ponens* rule. And thence one defines the relation *derivable from,* i.e. the relation *serially an immediate consequence of.*

### 6. *Non-Symmetrical Transitive Relations and Arrays*

A referent-class defined by a focus and a non-symmetrical transitive relation is an ordered ‘array’. *Higher than* is an example of such a relation, and the array of mountains higher than Mount Blanc, for instance, can be listed in order, with Everest at the top. Specially important are those arrays—I call them ‘chain-arrays’—defined by a non-symmetrical serial relation. The descendants of Queen Victoria constitute a chain-array—the ‘family tree’. The chain-array of theorems derivable from a given axiom-set can likewise be set out in a branching ‘tree’. The natural numbers—the serial successors of zero—make up another chain-array.

### 7. *Symmetrical Transitive Relations and Groups*

But the referent-classes I particularly want to consider are those defined by symmetrical transitive relations. I call such relations 'group-relations', and the classes defined by them 'groups'.

Among such classes there are important distinctions to be made. A class such as the *fellow-citizens of Henri Dupont* or the *siblings of Egbert Jones,* whose defining relation is a transitive fellow-relation, I call a 'fellow-group'. For reasons which I shall shortly explain, I call a class a 'proper group' only when it is not a fellow-group. Proper groups are either 'simple groups', defined by a symmetrical and simply transitive relation, or 'chain-groups', defined by a symmetrical chain-relation. I will first of all give some examples of both sorts of proper group.

*Equal in length to* is a simple group relation, and the class *things equal in length to the standard metre* is a simple group. Another such relation is *concomitant of,* where 'A is a concomitant of B' means: 'The phenomenon A occurs in any given situation whenever and only when the phenomenon B occurs in that situation' or 'the property A characterises an object if and only if the property B does'. A perfect syndrome, defined as the set of all those phenomena (or properties) that are concomitants of a given phenomenon (or property) is another example of a simple group. Yet another symmetrical and simply transitive relation is the relation *similar to,* where 'similar' is used in the set-theoretic sense in which one class is said to be 'similar' to another, if the members of the two classes can be paired off, one to one. A simple group defined by this relation is the class of all classes similar to the class of Gospel-makers—in fact the number four, according to Frege and Russell.

Consider next the symmetrical and non-transitive relation *within twenty yards of*. In terms of this one can define the symmetrical and transitive relation *serially within twenty yards of*. This is a chain-group relation, and a referent-class defined by it is a chain-group. Taking as focus, for instance, a particular tree in a forest, you may define the chain-group *trees serially within twenty yards of* this tree. If we lay it down that a tree counts as belonging to a forest if and only if it is within twenty yards of some other forest tree, then the chain-group we have defined coincides with the class of all the trees in this forest. For any two trees belonging to the same forest stand, according to our stipulation, in the chain-group relation *serially within twenty yards of* each other.

For our next example consider the symmetrical and non-transitive relation *crossable with,* defined as follows: Two local populations of plants or animals are said to be 'crossable' if they interbreed freely in nature, or would do so but for geographical or ecological barriers.[2] (It is a matter of biological fact that this relation is non-transitive. There do occur in nature series of populations where A is crossable with B, B with C, and C with D, but where A is *not* crossable with D.) On the basis of this non-transitive relation we can define the transitive relation *serially crossable with*. In terms of this, taking a local population as focus, we can define the chain-group *all those populations that are* serially crossable with this population. In so doing we define a 'biological species'[3]—for between any two populations belonging to the same biological species there holds the chain-group relation *serially crossable with*.

One more example. Consider the symmetrical non-transitive relation *similar to in very very many respects*. I will call this, for short, 'the relation *very like*'. Taking as your focus a particular plant, kept in a certain drawer in a certain herbarium, you can define the chain-group *all plants serially very like this particular plant*. In so doing, you define a particular 'morphological species', as taxonomists call it.[4]

## 8. *Properties of Groups as Such*

Let us now consider the logic of such classes; and first let us examine those features which, because they are consequences of the transitive and symmetrical nature of the defining relation, are common to groups of every sort.

(a) In every group the focus itself must be counted as a member. Thus the standard metre belongs to the group *things equal in length to the standard metre*; this forest tree belongs to the group *trees serially within twenty yards of this tree*; and Egbert Jones is himself a member of the group *siblings of Egbert Jones*. For let A be any member of the group *things that have R to F* (where R is the defining group-relation, and F is the focus). Since R is symmetrical, it follows that both F has R to A, and A has R to F. Since R is also transitive, and F,A,F is a series with respect to R, it follows that F has R to F. Thus F itself belongs to the class *things that have R to F*.

(b) Let us say that a set is 'integrated' by a relation, R, at any moment when all its members have R to each other, and none has R to any non-member. E.g., suppose that at noon today Smith, Jones, Mill and Green are all friends of each other, and that none of them is friendly with anyone outside that four-membered set. Then that set is, at that moment, integrated by *friend of*.

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[2] cf. A. J. Cain, *Animal Species and their Evolution* (1954), p. 94.

[3] *op cit.*, p. 86.

[4] *op. cit.*, pp. 52, 121.

Let us call a set 'omnifocal with respect to R' when to each of its members there corresponds a definition—in terms of R and of that member as focus—which designates the set. E.g., suppose that today there are four definitions, each of which designates the set [Smith, Jones, Mill, Green]—the definitions being: 'Smith and his friends', 'Jones and his friends', 'Mill and his friends', and 'Green and his friends'. Then that set is today omnifocal with respect to *friend of*. Plainly a set is omnifocal with respect to a relation whenever and only when it is integrated by that relation.

Having defined these two words for sets, let us now define them for classes. We shall say that a class is 'integrated' at any moment when its defining relation, R, integrates the set of all those that are then members of it. Since that set will then also be omnifocal with respect to R, we shall also say that the class is, at that moment, 'omnifocal'. E.g., suppose that those who, last Friday, were members of the class *Smith and his friends* constituted, on that day, a set integrated by and omnifocal with respect to the relation *friend of*. Then the class *Smith and his friends* was, last Friday, integrated and omnifocal. A class such as this can, of course, be omnifocal one day and not omnifocal the next.

But a group is, as such, necessarily integrated and omnifocal at all times. The class *things equal in length to the standard metre*, for instance, may change its membership from time to time, as metre-rods get broken and new ones made. But all things that belong to the class at any given moment are, at that moment, equal in length to each other, and none is equal to anything not then a member. And if, at any moment, X belongs to the class defined by 'things equal in length to the standard metre', then the set of things then members of that class is also, at that moment, defined by 'things equal in length to X'. (Since the focus of a group is itself a member, the definition need not read 'X and things equal in length to X', after the pattern of 'Smith and his friends'.) Likewise there may be changes, from time to time, in the membership of the chain-group *trees serially within twenty yards of the tree F*. But the trees that belong to it at any given time are all, at that time, serially within twenty yards of each other, and are not so related to anything not then a member. And if X belongs to it at a given time, then the set of trees that are then members is also defined, at that time, by 'trees serially within twenty yards of X'.

It can readily be shown that this must be so. Let R be a group-relation, and let A and B be any two members of the class *things that have R to F*. The symmetrical relation, R, holds, by hypothesis, between A and F, and also between F and B. But, since R is transitive, it follows that it also holds between A and B. So the defining relation must hold between any two members of a group. Let A be any member of the above class, and let X be any object which has R to A. Then X has R to A, which has R to F. It follows, R being transitive, that X has R to F; that is, that X is a member of the class. So an object which has the defining relation to any member of a group must itself belong to the group. Hence the defining relation of a group cannot hold between a member of the group and a non-member. Thus the set of objects which at any time belong to a group is at that moment integrated by, and co-omnifocal in respect of, the defining relation of the group.

### 9. *Fellow-Groups and Proper Groups*

So much for the logic of groups generally. I now turn to consider some of the main differences between different sorts of group; and first of all to explain why I exclude what I call 'fellow-groups', such as *the siblings of Egbert Jones,* from the category of proper groups.

The members of a fellow-group are, by definition, fellow-members of the same circle; and a fellow-relation is a derivative one, defined in terms of a simple non-transitive (circle-generating) relation. In a sense, then, a fellow-group is fundamentally a circle. Suppose you wish to identify the present members of a proper group. Then all you need to be told, in principle, is the defining relation and one present member. Any such member will do, and in tracking down the remainder of the set of present members there is no one object—whether member or non-member—that is essentially involved, or plays any special role. But it is different if you wish to identify the present members of a fellow-group. Given any present member—say Tessa Jones—of the class *siblings of Egbert Jones,* your first step in identifying the rest of the present members must in principle be that of finding out which are Tessa's parents; and the next step is to locate the other present members of the circle *children of Mr. and Mrs. Jones.* In this operation the elder Joneses play an indispensable part. Locating the present members of a fellow-group in principle involves locating the focus of the corresponding circle, and from that essential vantage-point locating the other circle-members. The 'omnifocality' of a fellow-group is a sham.

## 10. *Simple Groups and Chain-Groups*

Let us now consider the differences between simple groups and chain-groups. In any proper group the set of members at a given time can be defined in terms of anything that is then a member. And in the case of a simple group there really is absolutely nothing to choose, from this point of view, between one member and another. No one member of the simple group of metre-long objects is any more typically a metre long than any other; no set of four objects is any more typically a foursome than any other foursome. But in a chain-group, although any member could be used as the defining focus, one member is sometimes more suitable—more 'typical'—than another. The herbarium specimen which defines the morphological species is called the 'type' of the species, and is normally chosen so as to be typical of the species, rather than atypical. One could also express this point by saying that one would choose for the herbarium a plant which was 'central' to the species, and not 'peripheral'. For, unlike a simple group, a chain-group has a logical structure which gives point to these spatial metaphors. It is formally analogous to a bounded spatial whole, within which its members occupy, as it were, different positions.

Consider a spatial whole in the literal sense, such as an island. One could divide the island up, mentally at least, into a number of smallish roughly equal parts. The parts being adjacent, every part of the island is *serially adjacent* to every other part. So one could select one of these small land-areas as focus, and define the island as consisting of the chain-group of *all those land-areas serially adjacent to the given land-area,* just as one could select one plant as focus and define the morphological species as consisting of the chain-group *of all those plants serially very like the given plant.* The analogy may be developed further. One part of the island may lie between two others; one member of the species may be morphologically intermediate between two others. Two island-parts may be close together or far apart; two plants may be linked by a short or long chain of intermediate forms. If Warwickshire and Surrey are two parts of the island, such that, of the other parts, those nearer to Warwickshire than to Surrey outnumber those nearer to Surrey than to Warwickshire, then Warwickshire is a more central part of the island than Surrey is. Analogously, if A and B are two members of a morphological species, of which those members that resemble A more closely than they do B outnumber those that resemble B more than they do A, then A is a more typical—more central—member of the species than B is.

In thus stressing the analogy between a chain-group and a bounded continuum I do not wish to be taken as denying that there are also important differences. A species is a class of discrete items; a region of space is not. But a region of space can be *considered* as made up, in some arbitrary fashion, of adjoined sub-regions, with one of them as focus. *So considered,* the region *is* a chain-group of sub-regions. Thus all chain-groups, but no simple groups, have a quasi-spatial structure.

The two sorts of group differ also in the way one *checks* whether a given thing is or is not a member. To find out if a given rod belongs to the class of things equal to the standard metre, one has but to set it and the standard metre side by side; there is no need to examine anything else. The procedure is similarly straightforward with any simple group. But with a chain-group it is more complicated. To check whether a given plant belongs to the morphological species defined by an herbarium type may involve prolonged field investigation and the examination of very many plants. In the case of a chain-group one normally has to look and see whether a number of other things can be found that make up a suitable chain, linking the focus to the thing in question.

There is also a difference between the ways in which the membership of the two groups can wax and wane. The only way in which something can come to be or cease to be a member of the simple group of things equal in length to the standard metre is for it to get longer or shorter. But an object, indeed a whole host of objects, can cease or come to be members of a chain-group without, in themselves, altering in any way. Imagine a forest shaped like an hour-glass, with two circular wooded areas, one in the west and one in the east, joined by a rather narrow neck of wooded country. Select a tree, X, near the western edge, and define the chain-group *trees serially within 20 yards of X*. The class so defined consists of all the trees in the forest. Now cut a wide fire-break right through the central neck of woodland. The thousands of trees in the whole eastern half of the forest thereby cease to be members of the group you have defined, although in themselves they are quite unaltered. Cut another fire-break around X and its near neighbours, separating them from the rest of the forest, and your group dwindles to a handful.

### 11. *Groups and Clusters*

The rather artificial concept of a group has now served its purpose. It was introduced only as a stepping-stone to another and more difficult concept—that of what I call a 'cluster'.

Suppose that you make a practice of applying the word 'grock' only to things you take to be red, whereas my practice is to apply that word only to triangles. Then you, when we meet a red square, will be prepared to call it 'grock', but I will not: our different criteria of applicability will lead to conflicting application. And suppose that you tomorrow abandon your present criterion for applying 'grock', and adopt mine instead. Then one day you will call a certain object 'grock' and the very next day will refuse to apply that word to something exactly similar: the change in criterion of applicability will lead to a change in application. But now suppose that for some years the class *red things* (picked out by your criterion) and the class *triangles* (picked out by mine) happen to coincide in membership—that for some years it happens that all triangles and only triangles are red. Then throughout these years the fact that you and I have different criteria will never lead to any conflict in application: if one of us calls something 'grock', so will the other. And as long as the two classes coincide you may switch at will from one criterion to the other, without there being any change in your application of the word.

Now suppose that there are not two different specifications, but dozens or even thousands, which different people use as criteria in applying the same word; and suppose that people often switch from one of these criteria to another. This could easily result in constant conflicts and constant changes in application. But what if it so happens that for many years the dozens or thousands of classes, defined by all these different specifications, all coincide in membership? While this class-coincidence lasts the fluctuation and diversity of criteria will hardly be noticeable, and could pass quite unnoticed, for there will be no conflicts and no changes in application. At any given moment there will be a set agreed by all to be the set of things to which the word applies.

Now the above situation readily arises whenever a sizable group remains for a long time perfectly stable—without any change in membership. Suppose, for instance, that the group *things equal in length to the standard metre* remains quite stable for a whole century. Then the set of things which at that time are members of the group has, throughout the whole century, at least as many definitions as it has members. For if X and Y are any two members of the set then, throughout the century of stability, the class *things equal in length to X* and the class *things equal in length to Y* coincide in membership with each other and with the class *things equal in length to the standard metre.* So it will not matter if, throughout that period, Smith applies a certain word according to the criterion of equality to X, Jones uses the same word with the criterion of equality to Y, and Robinson uses it with the criterion of equality to the standard metre. Nor will it matter if each person keeps on changing his criterion for applying the word, provided that his definition always has, for its focus, some member of the set. There will be no conflicts and no changes in the application of the word. Hence there will be no need, in this situation, for everyone to agree on one criterion as the 'correct' one to follow in applying the word, and no need for any given person always to stick to the same criterion. For purposes of smooth communication and co-operation in practical affairs any criterion will do, so long as it successfully picks out the set in question.

Thus it can come about that your criterion for applying a word is regarded as all right, whatever it may be; provided only that it leads you to apply the word to the same set of things that other people, following their possibly different criteria, apply it to. When an omnifocal set is thought of in this way, as definable in terms of any member, any such definition being regarded as just as correct as any other, I call the set a 'cluster'. I call it a 'chain-cluster' if the integrating relation is a chain-relation, and a 'simple cluster' if not.

The above is not a definition, but only a preliminary description of the sort of thing I call a 'cluster'. I have so far only considered a stable situation, and until we have considered the ways in which a cluster can change, while yet remaining 'the same cluster', the notion will not be fully explained. But before we turn to this it will be as well to give some preliminary examples.

Consider the group defined by 'classes that are similar to the class of Gospel-makers' (where 'similar to' means 'can be exactly paired off with, member for member'). This class is, in fact, the class of foursomes—the class designated by the word 'four'. But one could not say that the word 'four' is simply synonymous with 'the class of classes similar to the class of Gospel-makers', or with 'the class of classes similar to the class of legs of the horse'. But these two definitions, and countless others, would all equally well serve to pick out the class of foursomes. If X is any foursome then the class of foursomes is definable by 'the class of classes similar to X', and we do not regard any one of these definitions as any more of a definition of a foursome than any other. Thus our concept of the class of foursomes is the concept of a cluster, not of a group. And, since the set-theoretic relation *similar to* is a simply transitive one, foursomes constitute a simple cluster.

Suppose that, within a certain forest, we select at random a certain tree, X, in terms of which we set up the definition 'trees serially within twenty yards of X'. The class we thereby define is, in fact, the class of trees that make up the forest. But our concept of 'this forest' is not the concept of the class that fits this definition. Thousands of other such definitions would be just as good, just as correct. We do not think of a forest as an aggregate centred round one particular focus, but as a chain-cluster of trees, with no special focus. Likewise a field naturalist who has learnt, by long experience, to recognise on sight members of a diversified 'polytypic species' does not normally think of the species as a group of forms serially related to a certain focal form. He thinks of it as a chain-cluster of forms which is not essentially defined in terms of any particular one of them.

It is, however, only proper groups—simple and chain-groups—that correspond, in the above way, to clusters. For only with them does it come about that there is a large number of possible criteria which different people could adopt, all such criteria being in principle equally good for the purpose of picking out the members of the set. Suppose that a certain man, Ali, has three wives, Zuleika, Fatima, and Ayesha. We could regard that set of three women as a fellow-group—as defined, for instance, by 'fellow-wives to Zuleika', or by 'fellow-wives to Fatima'. Or we could regard that same set as a circle, defined by 'wives of Ali'. But the different definitions which would all serve to pick out the same set of women are not, in this case, all on a par. To find out, for instance, whether or not a given woman, X, belonged to the class *fellow-wives of Zuleika* one would first have to find out who Zuleika's husband was, and then check whether or not X belonged to the class *wives of Ali*. For the picking out of this set, therefore, one criterion is fundamental, that of being wife to Ali.

## 12. *Essential and Accidental Clusters*

Whenever a set of objects is, over a period of time, integrated by a certain relation (other than a fellow-relation), we could come to think of that set as a cluster. When a number of objects all have a group-relation to a given object it follows logically, as we have seen, that the whole set must be integrated by that relation. So we could say that a set integrated by a group-relation is 'essentially integrated', and also say, if the set happens to be thought of as a cluster, that it is an 'essential cluster'. We also saw that a set of objects might, over a period of time, be 'accidentally integrated' by some relation which is not a group-relation. (E.g., Smith, Jones, Mill and Green might all be friends of each other, and none of them friendly with anyone else.) If such a set came to be thought of as a cluster it would be what we might call an 'accidental cluster'.

Consider, for instance, a set of properties or phenomena that are all concomitants of each other. Since *concomitant of* is a group relation, such a set is essentially integrated by it, and if we think of that set as a cluster—as a perfect syndrome—it will be an essential cluster. Consider now the relation *near-concomitant of,* which, if we want to make things precise, we might define as follows: A is a near-concomitant of B if and only if the relative frequency of cases in which A and B are both present, among cases where at least one of them is present, is 99% or over. This relation is clearly non-transitive: if A is a near-concomitant of B, and B of C, it does not follow that A is a near-concomitant of C. So the set of things which are near-concomitants of a given thing need not all be near-concomitants of each other. Nevertheless a set of properties or phenomena might all happen to be near-concomitants of each other, and none of them a near-concomitant of anything else. And, if we thought of such an accidentally integrated set as a cluster, it would be an accidental cluster.

## 13. *How Clusters Can Change*

The difference between the logic of clusters and that of groups comes out really sharply when one considers a changing situation. I shall now discuss this is some detail in terms of a very simple model, before going on to apply the cluster notion to a number of traditional philosophical issues.

Suppose that twenty-six ships, whose names are A, B, C, D, etc., are at sea in war-time, maintaining radio silence. Each is in visual contact, by means of signal lamps, with at least one of the others, so that every one of the twenty-six is serially in contact with the remaining twenty-five, but with no other ship. If that situation were to last for any appreciable time we should think of the twenty-six ships as constituting a single enduring entity—a cluster of ships. Since we might want to refer to this cluster on several occasions we might perhaps give it a proper name, such as 'Tom'. (Remember that weather-forecasters sometimes give names like 'Clara' to cyclones and typhoons.)

If a storm scattered our twenty-six ships, so that none was any longer in contact with any of the others, we should have to say that Tom had ceased to exist. Or suppose it came to pass that the thirteen ships A to M were serially in contact with each other, and that the ships N to Z were also serially in contact, but that no ship of the first set was in contact with any member of the second. In that case, too, we should have to say that

Tom was no more—that it had split into two new clusters, neither of which could properly claim to be the original Tom. If the split was only very temporary, and the whole twenty-six were very soon again in serial contact, we might well say: 'For a short while Tom split into two, but now it's back to normal again.' Plainly the notion of an individual cluster, to which we could give a proper name, would not get a grip unless there were a certain minimum degree of stability: too frequent and too complicated fissions and fusions would defeat us. (Compare the clouds in a windless summer sky—each an enduring chain-cluster of droplets—with the writhing, splitting-and-fusing mass on a windy day in winter.)

But the stability need not be absolute. If A were to drift away from the other twenty-five ships, while these latter still remained serially in contact with each other, we should no doubt think of the cluster, Tom, as still existing. We should say that Tom had lost one of the ships it used to consist of, and now consisted only of the twenty-five ships B to Z. And if, later on, these twenty-five were joined by two new ships, $\alpha$ and $\beta$, we should say that Tom had grown by the addition of two new ships, and now consisted of the twenty-seven ships $\alpha$ and $\beta$ and B, C, D, E, etc. If one of these twenty-seven—the ship M, say—were to drift away, leaving the other twenty-six serially in contact with each other, we should say that Tom was still there, but that M no longer belonged to it. But this means that, of the twenty-six ships, A to Z, of which Tom at present consists, not one is essentially a member of Tom. Tom could still be Tom, even without it.

What holds for actual changes in the spatial position of ships holds also for changes in our beliefs about the relative positions of ships. Suppose that you believe that the ships A, B, C, D, E and F, with some twenty or so others, constitute a set which is integrated by the relation *serially in contact with*, and that you call this cluster by the name 'Tom'. Now suppose that you discover that your sources of information were defective—that the ship A is not, as you had thought, serially in contact with any of the ships B, C, D, E or F, but that these are all, along with twenty or so others, serially in contact with each other. In such a case you would still give the name 'Tom' to the cluster consisting of B, C, D, E, F and the rest. And you would say: 'I used to think, mistakenly, that A was a member of Tom, but I have since discovered that it is not.' Likewise you might discover that $\alpha$, which you had thought to be far away, had all along been in contact with B, C, D, E, F and the rest. You could say, if so: 'I didn't realise that $\alpha$ was a member of Tom, but now I have discovered that it is.' And what applies to A or to $\alpha$ applies to any other supposed member or non-member of Tom. Hence no ship which is, or is thought to be a member of Tom, is essentially a member: and no ship which is, or is thought to be a non-member, is essentially a non-member.

## 14. *Archetypes and Conceptions*

It is often held that the meaning which a person gives to a word, the concept he expresses thereby, is identical with his criterion for applying it. This criterion, stated in his definition of the word, is a specification or description such that he would be prepared to apply the word to something if and only if he thought it met the specification. It is, in Locke's words, an 'archetype', an 'original pattern in the mind' to which 'we refer for the distinguishing of particular beings under names'.[5] The view that all meanings and concepts are of this sort I shall call the 'archetype theory'. It holds that you and I give different meanings to the same word—express different concepts thereby—if our criteria for applying it differ; and that whenever you change your criterion for applying a word you thereupon give the word a different meaning. A word's having a commonly accepted 'standard' meaning consists, on this view, in the fact that different speakers all follow in applying the word the same 'standard' criterion. For every such word there is *the* correct criterion, stated in *the* correct definition.

This theory seems to give a correct enough account of the meaning of certain words, for instance of 'triangle' (a favourite example in discussions of meaning and definition). But it seems much less plausible when applied to cluster-names such as 'Tom'.

Assume that the ship-cluster commonly known as 'Tom' consists, in fact, of the twenty-six ships A to Z. Suppose that your beliefs on the matter are as follows: you think that people who use the word 'Tom' refer thereby to a ship-cluster; you think that $\alpha$, B, C, D, E and F belong to it; but you do not know what other ships, if any, are members. These beliefs, we could say, make up your 'conception' of Tom. It is not a very full conception, though it is not as sketchy a conception as would be possessed by one who believed no more than that the cluster called 'Tom' contained B and C. Moreover your conception is a slightly erroneous one: you are wrong in thinking $\alpha$ to be a member. But it is a conception *of Tom*. For these beliefs of yours would guide you in your application of the word 'Tom', and they would on the whole guide you rightly. Let us try to see how.

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[5] *Essay Concerning Human Understanding,* Bk 3: Ch. 5 §12.

Your problem is to decide whether a given cluster is Tom or not. What you have to go on is a set of facts about individual ships, each to the effect that a certain ship is or is not a member of the given cluster. The only such facts that will be any use to you, given your conception of Tom, will be facts about one or other of the ships $\alpha$, B, C, D, E or F. Facts about G and $\beta$, for instance, would not mesh with your conception of Tom, though they could be of use to someone with a different conception.

Given a conception which, as in our example, is wrong on one point, and given that you have no more than one relevant ship-datum to go on, it is possible though improbable that you would give the wrong answer to the question: Is this Tom or not? If your sole fact is that the cluster contains $\alpha$, you will wrongly call it 'Tom': if your sole fact is that the cluster does not contain $\alpha$, you will conclude, perhaps wrongly, that it is not Tom. But if your sole fact concerns any one of B, C, D, E or F you will give the right answer.

Even if you go wrong in such a case, your error will be corrected as soon as more relevant facts come in. For you will not go wrong provided the facts you have to go on are more numerous than the points on which your conception is incorrect. Suppose you know two facts about the given cluster. If they are to mesh with your conception they must be facts about two of $\alpha$, B, C, D, E and F. Assume that they relate to $\alpha$ and B. Now you could not discover that these two ships both belong to the cluster before you, for on our assumptions no cluster contains them both. If you know that the given cluster contains neither $\alpha$ nor B you will conclude, rightly, that it is not Tom. There remains the possibility that the given cluster contains one of these but not the other, so that your two facts seem to point to opposite conclusions. In that case you would still not go wrong. For your immediate conclusion is that your conception of Tom must be wrong, and you would have to suspend judgment on the cluster before you until you had many more facts.

When you have amassed, say, six relevant facts you will find that your data, though perhaps not perfectly unanimous, point overwhelmingly to one definite answer. For on our assumptions there are then just three possibilities. You will either find (1) that the given cluster contains none of the ships involved in your conception of Tom or (2) that it contains $\alpha$ but none of B, C, D, E or F, or (3) that it contains all of B, C, D, E and F, but not $\alpha$.

In cases (1) and (2) the obvious conclusion is that the cluster is not Tom, and in case (3) that it is.

From case (2) or case (3) you would learn not merely that your conception of Tom was wrong but also what emendation was called for. Given that $\alpha$ is not in contact with any of B, C, D, E or F it follows that people who use the word 'Tom' either refer to a cluster not containing $\alpha$, or refer to a cluster containing none of B, C, D, E or F. If the former is true your conception is slightly in error; if the latter, there is almost nothing right about it. If we may assume that a slight error is not very improbable, whereas the chances of really wholesale errors are pretty well negligible, then we may confidently accept the former alternative—that 'Tom' refers to a cluster not containing $\alpha$. Your revised conception of Tom must therefore be of a ship-cluster comprising, among others, B, C, D, E and F.

In case (3), moreover, you would actually be in touch with Tom itself, and so would be able, with luck, to look and see what other ships were serially in contact with the known ones. If you found, for instance, that A and G were also in touch with these, your conception of Tom would be enlarged. As a result of this empirical discovery you would now think of Tom as comprising, among others, A, B, C, D, E, F and G. Notice that in such cases you would be led to revise or enlarge your conception of Tom *not* by a study of other people's usage of 'Tom', but by learning new facts about what ships are in contact with what.

We have so far considered only a conception wrong on one single point. But similar considerations apply to conceptions wrong on two or more points, provided that they are mainly correct. Suppose you think that 'Tom' refers to a cluster comprising, among others, $\alpha$, $\beta$, C, D, E, F, G, H, I, J and K. If you decide about a given cluster on the basis of two facts only you could go wrong, for those two facts might happen to relate to $\alpha$ and $\beta$. But with three relevant facts you would be safe. If you found the cluster to contain $\alpha$ and $\beta$ but not C, for instance, you would know that your conception was in some way erroneous and that you needed more facts about the cluster. If, later, you found it to contain $\alpha$ and $\beta$ but none of C, D, E, F, G, H, I, J or K, you would know it was not Tom, and know how to revise your conception.

Thus a conception, even if erroneous, can guide you rightly in applying the word 'Tom'. You can assess the risk of its misguiding you in a given situation, if you can assess the chances of your conception containing as many errors as you have, in that situation, facts to go on. To do this generally you need to know the probabilities of your conception's containing errors of various sizes. And to do this you need general empirical knowledge about ship-clusters. For the chief source of erroneous conception is that a conception, once quite correct, gets out of date and inaccurate owing to an unheeded change in the cluster or in the available knowledge concerning it. Hence you need to know how stable ship-clusters are, in general—what is the chance of such a cluster changing or of our discovering its composition to have been other than the experts had thought.

A cluster can be thought of as an entity and given a name only if it is stable enough for the probability of a slightly erroneous conception to be not high and for that of a wildly erroneous full conception to be negligibly small. That this should be so in the case of Tom is the general pre-supposition of the word 'Tom' having significance as a cluster-name.

From all this it will have become evident how very different is the sort of meaning which 'Tom' has from the meaning of a word which is used with an archetypal criterion. Let us summarize the main differences:

(1) There are many thousands of possible conceptions, some full, some sketchy, some quite correct, some slightly erroneous, but all capable, if used with due circumspection, of guiding you aright in your use of 'Tom'. And different speakers will in fact be guided by different conceptions, and will change their conceptions from time to time. From the point of view of the archetype theory it is natural to confuse all these different guiding conceptions of Tom with different archetypes, and to conclude accordingly that the meaning of 'Tom' fluctuates wildly from person to person and from time to time.[6]

(2) A picture or a description can represent what it is a picture or description of either fully or sketchily, either correctly or erroneously; and to correct any errors in it one must examine the thing it purports to represent. In these respects, as we saw, a conception is like a representation. But of an archetype, unlike a conception, it does not make sense to say that it is full or sketchy, correct or erroneous. Nor does it make sense to speak of finding out, from an examination of things, how an archetype must be amended. For an archetype does not purport to represent or to conform to anything. It is rather *that to which* things are required to conform if they are to be called by a certain name.

Hence it is wrong to identify a conception with an archetype.

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[6] cf. Locke on names of 'substances', see *Essay* 3-6-26; 3-6-31; 3-9-13; and Mill, *Logic* 4-5-1; 1-8-4.

It is never an archetype that guides a person in applying 'Tom'. But if 'Tom' has no archetypal criterion it follows, on the archetype theory, that 'Tom' has no meaning.[7]

But in a natural and proper sense of the word 'meaning', 'Tom' has a meaning, and a quite constant and uniform one, despite all the variations in conception.

### 15. *Cluster-Statements and Item-Statements*

In order to throw light on the general problem of the relations between cluster-statements and item-statements, let us now consider inferences from simple or compound ship-statements to statements about Tom, and vice-versa. By 'simple' ship-statements I mean affirmative or negative statements about named individual ships, such as 'A is not in the Atlantic' or 'B and C have been in contact for three days'. By 'compound' ship-statements I mean truth-functional compounds of these.

To begin with, no simple ship-statement ever entails or is entailed by a Tom-statement. No single ship, as we saw, is either essentially a member of Tom or essentially a non-member. It is, for instance, not self-contradictory to say 'A is not a member of Tom', so you can without contradicting yourself say 'Tom is in the Atlantic but A is not', or say 'A is in the Atlantic but Tom is not'. Hence 'A is in the Atlantic' neither entails nor follows from 'Tom is in the Atlantic'.

But you could, given the truth of a simple ship-statement, have empirical grounds for asserting a Tom-statement. And likewise a Tom-statement could empirically imply a ship-statement. Suppose you have often used the word 'Tom' in conversation with others without any apparent break-down of communication: so far as you can tell you apply it in just those cases in which everyone else does. Or suppose that the meaning of 'Tom' was explained to you by a normally reliable informant. In either case you have inductive grounds for thinking that your grasp of the meaning of 'Tom' is quite adequate—that your conception of Tom, provided that you are duly cautious when arguing on the basis of very slender evidence, will always lead to the correct use of 'Tom'. But to be reliable in this way a conception needs to be either completely correct or else fairly full and at least mostly correct. (We have seen how a slightly erroneous conception could be adequate for correct application, but you would soon go wrong if the error were more than slight.)

Let us suppose, then, that your conception of Tom is of a cluster containing (among others unknown) A, B, C, D, E, F, G, H, I and J, and that you have inductive grounds for thinking that this conception is adequate for the use of 'Tom', and so at worst is only slightly erroneous. Then you have empirical grounds for thinking that A is very probably a member, grounds for thinking that B is, and so on. Hence, given the premiss that Tom is in the Atlantic, you have empirical grounds for thinking that A, being probably a member of Tom, is probably in the Atlantic. Likewise 'A is in the Atlantic' empirically implies 'Tom is in the Atlantic'.

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[7] cf. Mill on proper names, *Logic* 1-2-5; 1-8-1.

What of compound ship-statements? Given as premiss the conjunction 'A, B and C are all in the Atlantic' you obviously have stronger grounds for 'Tom is in the Atlantic' than you would have, given only a simple ship-statement. And 'Tom is in the Atlantic' equally obviously gives stronger support to the disjunction 'At least one of A, B and C is in the Atlantic' than it does to the simple 'A is in the Atlantic'. You can be led into error in inferring from the conjunction or to the disjunction only if your conception is wrong on each of three distinct points. On the other hand, 'Tom is in the Atlantic' gives weaker support to the conjunction 'A, B and C are all in the Atlantic' than it does to 'A is in the Atlantic'.

It is plain that the above implications are all empirical, not entailments. You do not contradict yourself in saying 'A, B and C are all in the Atlantic but Tom is not', nor in saying 'Tom is in the Atlantic but neither A, nor B, nor C is'. It is not self-contradictory to deny that Tom contains any of A, B or C. But what if, having the conception of Tom described above, you were to say 'A, B, C, D, E, F, G, H, I and J are none of them members of Tom'? Since these ten ships are the only ones that enter into your conception, would you not be contradicting yourself? And, if so, would it not follow that the tenfold conjunction 'A, B, C, D, E, F, G, H, I and J are all in the Atlantic' entails 'Tom is in the Atlantic', and that this latter statement entails the corresponding tenfold disjunction?

But surely it is quite conceivable, though on our assumptions most unlikely, that your conception should be entirely wrong and that Tom—the cluster known to the general public as 'Tom'—should contain none of these ten ships? Then let us construct a more extreme case. Consider a certain conjunction of hundreds of simple ship-statements. To save time I shall give a general description of it, rather than state it in detail. It is a conjunction which could be summarized as saying that for the past ten years the twenty-six ships, A to Z, with some others, have been in various parts of the world but all the time serially in contact with each other, and that they are now all together in the Atlantic. Would not this huge conjunction entail 'Tom is in the Atlantic'? And would not 'Tom is in the Atlantic' entail the corresponding huge disjunction?

The case for saying so is as follows: suppose that this huge conjunction in fact gives a true account of ten years in the history of the cluster commonly known as 'Tom', and that someone asserted it and then added 'But Tom isn't in the Atlantic'. What should we say of him? Surely that he had not grasped the meaning which is commonly given to the word 'Tom', or that he was, for reasons of his own, using this word eccentrically? And is not this just the sort of thing we should say of a person who asserted that a certain figure was plane, rectilinear and three-sided, but then added 'It isn't a triangle'?

But this entailment by the huge conjunction, if indeed it is an entailment, is somewhat unusual. A small conjunction empirically implies the Tom-statement, and a huge one apparently entails it; but there could hardly be a precise point at which, by the addition of just one item, an empirically implying conjunction is transformed into an entailing one. Compare the following example: Numerals are written on a certain page, and you have empirical grounds for thinking that if any whole number below 21 is written there then most of the numbers below 21 will occur. Then the small conjunction '5 and 14 both occur' will empirically imply 'Most of the numbers below 21 occur'. But the latter statement is entailed, not empirically implied, by the large conjunction '1, 2, 3, 4, 6, 7, 8, 9, 11, 12, 13, 14, 16, 17, 18, 19 and 20 all occur'. And owing to the vagueness of 'most' there will be no sharp line between conjunctions that empirically imply and those big enough to entail. So far our two cases run parallel. But whereas 'most' is merely vague our Tom-statement has 'open texture'.

One can remove the vagueness of 'Most of the whole numbers below 21 occur' by arbitrarily deciding, for instance, that this statement is to count as true if and only if at least fifteen of the twenty numbers below 21 are written on the page. Now there are exactly 21,700 different ways of selecting at least fifteen objects out of twenty. So our statement, made more precise by this arbitrary stipulation, would be entailed by any one of 21,700 conjunctions, but by no others. The point is that these entailing conjunctions can all in principle be foreseen by anyone who understands the stipulated meaning of our statement. There could be no question of a person's empirically discovering that there was some other conjunction, outside these 21,700, which also entailed the statement.

But Tom, while still continuing to be the same cluster, can gradually change its membership over the years, losing a lot of the original members and coming to include, at the end, a whole lot of ships that no one had ever heard of in previous times. No one, in principle, can foresee all the developments that will or might take place, what ships might come to be members. So ten years hence there might be a different huge conjunction which will then 'entail' that Tom—the same Tom as before—is again in the Atlantic as it was ten years previously. But we cannot now foresee all possible 'entailing' conjunctions of this sort, if only for the reason that we do not now possess a list of all past, present and future ships. No arbitrary stipulation can get round this, if 'Tom' is to be the name of a cluster.

Is 'entails' the right word to use here? It does not seem quite to fit. But nor does 'empirically implies'. Have we here a type of implication that eludes the traditional classifications?

## 16. *Clusters Reducible to Items?*

If we were making a list of all the things in the universe, should we include Tom as well as all the ships? Are facts about Tom further facts over and above facts about individual ships? Or are Tom-statements reducible to ship-statements?

Several considerations point towards the latter alternative. Tom-statements, it would seem, 'semantically presuppose' ship-statements. A person could know how to use ship-statements without understanding a single Tom-statement. But how could we teach someone to use Tom-statements without getting him to grasp such things as what it is for the ship A to be present, to be in the Atlantic, to be in touch with B? The meaning of Tom-statements must be explained or shown in terms of individual ships. Secondly, Tom-statements 'epistemically presuppose' ship-statements. One confirms a Tom-statement by ascertaining the truth of a number of ship-statements. And this confirmation is direct, in the sense in which one directly confirms that it is raining by feeling the rain on face and hands, whereas one confirms this only indirectly by seeing through the window that people are carrying umbrellas. We do not envisage any way of checking a Tom-statement more direct than that of observing particular ships and their positions. For that would be to envisage the possibility of observing the cluster itself, without observing the ships. And finally it seems that Tom-statements 'ontologically presuppose' ship-statements. Whereas we could imagine a world in which there were ships but no ship-clusters, it is self-contradictory to suppose that the world might contain ship-clusters but no ships.

But if these reasons convince us that a world that contains ships does not also contain an extra entity, namely Tom, we are at once faced with the problem: what, then, is the significance of statements ostensibly about Tom? If there is no Tom for them to be about, then presumably they must be, in some way or other, about individual ships. But in what way? Several answers are possible.

It could be said that Tom is simply definable in terms of individual ships. There are, as we have seen, many different conceptions by which different people might be guided to the same correct application of the word 'Tom', and a person asked to explain what 'Tom' means could hardly do better than give his conception. In a certain sense of the word we could call any such reply a 'definition', for it would certainly put a hearer into the way of using the word correctly. But in this sense of the word there is no one correct 'definition' of Tom: there are many possible ones, and a person could switch from one to another while all the time meaning the same thing by 'Tom'. Furthermore, any such 'definition' is empirically corrigible: however sound it seems we cannot be sure that future discoveries about ships will not show it to be erroneous. Hence you might praise such a definition as 'real'—as 'of the thing itself'—but you could not claim that it indicates any expression as *synonymous with* 'Tom', in the way that 'plane, rectilinear, three-sided figure' is synonymous with 'triangle'. And our problem of the meaning of statements which are only ostensibly about an entity called 'Tom' seems to call for a definition by synonymous phrase.

It might be suggested that Tom, though not definable in isolation, is nevertheless 'contextually definable' in terms of ships—that for every complete statement ostensibly about Tom there is always a synonymous ship-statement. But any compound ship-statement that is proposed as having the same meaning as 'Tom is in the Atlantic' would have to mention by name the members of a certain set of ships—presumably the set of present and past members of Tom, or some sub-set of these. And 'Tom is in the Atlantic' could still have been true, even if Tom had contained a different set of ships as members. It could still have been true, therefore, even if the proposed translation in terms of individual ships had been false.

But could we not hold that Tom-statements are nevertheless reducible to ship-statements, in the sense that for every Tom-statement there is at least one conjunction of ship-statements that entails it? A case can be made out, as we saw, for saying that certain huge conjunctions of ship-statements may sometimes 'entail' a Tom-statement. But, as we also saw, there are serious objections, on the score of open texture, to saying this. Ten years hence 'Tom is in the Atlantic' will mean, but for the different time-reference, just what it means today. What huge conjunctions will then 'entail' it? We cannot now say, but in ten years' time we shall be in a position to say. Can that which so obviously hangs on contingency and experience be called 'entailment'? Should we not say that Tom-statements and ship-statements are on two different and distinct levels of language?

This whole dialectic has a very familiar ring. We have met this tug and counter-pull of reason before—in discussions of the ontological status of scientific entities, for instance, and of the logical relations between higher-level theoretical statements and lower-level observation-statements. We have met it, too—alongside other perplexities—in disputes concerning realism and phenomenalism. I suggest that with any cluster the teasing ontological problems come up in a specially simple form, and that you have uncommonly simple instances of 'transilient' logical relations—those perplexing relations that overleap the boundaries between one level of language and another. I suggest that unless you can see clearly into the logic of 'Tom' you cannot really hope to give satisfactory answers to a lot of more complex metaphysical questions.

Having worked through this simple example in some detail, let us now consider its bearing on some traditional problems.

## 17. *Material Bodies as Clusters*

A material body, such as a knife, a house or a human body, is a qualitatively and spatio-temporally continuous whole. A knife, for instance, occupies at any instant a certain region of space, and if it ever occupies two different places at different times then at some intermediate time it occupies some intermediate place. At any moment certain attributes, states, actions are predicable of it, and if different things are predicable at some later moment then, roughly speaking, at some intermediate time intermediate predicates apply. It can be thought of as a four-dimensional space-time 'worm', continuous and bounded. Now any such continuum, as we have seen, can be considered as a chain-cluster of parts. Any two adjacent parts will be temporally very close, and either very close or identical in respect of position and of the other predicates applicable. Let us express this shortly by saying that any two adjacent parts will be 'continuous'. The relation of 'being continuous', thus defined, is symmetrical but not transitive. In terms of it we can define the chain-relation *serially continuous with*. This relation will hold between any two segments, adjacent or not, of a space-time worm. Or, to drop the *sub specie aeternitatis* mode of speech, any two stages or episodes in the history of a material body will be serially continuous with each other.

A single episode in a body's history could be described by saying: at such-and-such a time such-and-such a place was occupied by a body of which so-and-so was then predicable. An episode in the history of a human body, for instance, could be described by saying: at 11 p.m. yesterday at the left of the fire-place a human body, six feet tall, was sitting, talking, smoking, etc. Another such episode could be described by: at 7 a.m., today, under the shower in the bathroom, a human body was standing and singing. These would count as episodes in the history of the same body if and only if they were serially continuous with each other. Thus we may regard a particular human body, for instance the one to which we give the name 'Tony', as a cluster. It is a cluster to which ever new members are added, instant by instant, as Tony passes through successive stages in his life-history. And it is a cluster from which nothing that has once been a member can ever subsequently depart. One's past history is past, and cannot now change.

But one's conception of Tony can change. In fact all those general points which were made about conceptions of the ship-cluster Tom can be equally well applied to conceptions of Tony. At any given moment you have a certain conception of Tony: you think of him, perhaps, as the man born in a certain room in Paris at a certain moment during 1910, as the man who got married in Madrid in 1930, as the man who shook your hand at a certain street-corner one day in 1940. You believe, that is, that a certain three episodes occurred: a birth at a certain moment in a certain Paris room, a certain Madrid wedding, a certain street-corner hand-shake. Let us, for brevity, refer to these three episodes by the letters 'A', 'B' and 'C', respectively. You believe, then, that A, B and C all occurred. You believe that these three are all serially continuous with each other, and with numerous other episodes unknown to you. And you think that when people use the word 'Tony' (in a certain sort of context) their remarks have reference, as do yours, to the cluster comprising A, B and C, together with all other episodes serially continuous with these. As time goes on you learn of other episodes—a certain car accident, D, a certain speech-making, E, and a certain song-singing, F—which you take to be serially continuous with each other and with A, B and C. Thus your conception is enlarged. You now think that 'Tony' has reference to that cluster of serially-continuous episodes which comprises, among others unknown, A, B, C, D, E and F. Yet despite this change in conception there will be no change in what you mean by 'Tony'.

Suppose that another person's conception is in terms of the episodes G, H, I, J, K, L and M; that G, H, I, J, K and L are all, in fact, serially continuous with A, B and C, etc., but that M is not. (Tony was not involved in episode M.) Then this is a conception of Tony. The person who has it means by 'Tony' just what everyone else does, despite the fact that his conception differs widely from yours, and is even slightly erroneous.

If asked to say 'what you mean' by the word 'Tony' you would reply by giving, in whole or in part, your current conception of Tony. Such a reply might perhaps be called 'your definition' of Tony. But any such 'definition' is open to correction in the light of further or more reliable information about what happened in this or that place at this or that time. Even if your conception of Tony is of one who, among other things, was born in Paris—even if most people think that Tony was born there—it is nevertheless not self-contradictory to say, 'Tony was born not in Paris but in Bermuda'. No episode which is, or is thought to be a member of an episode-cluster is essentially a member of it; and no non-member is essentially a non-member. Open texture, corrigibility of definition, is a feature of the logic of proper names.

Because of this fact, and because different definitions could be given by different people, and by the same person at different times, without 'Tony' fluctuating in meaning, we may fairly conclude that there is no one correct archetypal criterion for the use of this word. So if we think, as J. S. Mill seems to have done, of the meaning of a word as the archetypal criterion for its use, we are bound to accept his conclusion that 'Tony', and proper names generally, have no meaning—are 'meaningless marks.'[8] And if, as Wittgenstein seems at times to do, we identify the meaning of a word, for a given speaker on a given occasion, with the criterion he happens to be then following, we shall have to conclude that 'Tony', and other proper names, have a wildly fluctuating meaning.[9] A person following this line of thought might argue: if I explain what I mean by 'Tony' by saying 'I mean "the man who sat on the piano at last night's party" ', whereas you mean by 'Tony' 'the president of the chess club', surely you and I attach different meanings to the name 'Tony', even though it may *happen* to be the case that the man who sat on the piano *is* the chess-club president.

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[8] *Loc. cit.*

[9] cf. *Philosophical Investigations* §79. '. . . If I had given a definition of the name in such a case, I should now be ready to alter it. And this can be expressed like this: I use the name "N" without a *fixed* meaning . . .'.

The answer, of course, is that the standard usage of 'mean the same' is such that in a case of that sort we two are properly describable as 'meaning the same'. And if someone protests: how *can* whether you and I mean the same turn on some contingent fact of which neither of us may be aware? he will very likely be told: you are confusing sense and reference—whether you and I are *referring* to the same thing *can* depend on some contingent fact unknown to us; whether you and I give the same *sense* to our expressions cannot. I shall presently be arguing that what would usually be thought of as the *sense* of certain expressions other than proper names is often really much more like *reference* than is commonly supposed.

Is Tony, considered as a space-time worm, an ultimate constituent of the Universe? Or are those philosophers right who have thought the ultimate elements were, in some sense or other, 'events'? If material bodies can fairly be regarded as clusters of serially continuous segments, it would seem possible to imagine a world in which there were no space-time worms but only isolated segments—or rather only entities of the sort that would be spoken of as 'segments' if they were linked up into clusters. But there could not, of course, be space-time worms in a world that contained no 'segments'. On the other hand there are serious objections, which we shall consider later, to the claim that Tony semantically or epistemically presupposes 'episodes', in the way that Tom semantically and epistemically presupposes individual ships.

## 18. *Kinds as Clusters of Classes*

The clusters Tom and Tony are individuals, the items composing them are particulars, and the integrating relation is, in both cases, at least partly spatial. But in clusters which are not individuals, and not composed of spatially-related particulars, the same pattern is found.

Consider the case of a natural kind, such as a morphological species. Two individuals count as members of the same morphological species if and only if they are serially very like each other. Hence the members of such a species constitute a chain-cluster of individual organisms, integrated by that relation. But it will not do to say that a morphological species *is* such a chain-cluster of individuals.

You understand the personal name 'Tony' provided you know which person the name refers to. You understand the proper name 'The Cabal', which designates a certain set of five men, provided you know which these five were, namely, Clifford, Arlington, Buckingham, Ashley and Lauderdale. But if you know *which* individual organisms belong to the species *Eiseniella tetraedra* and which do not, it does not follow from this alone that you understand the meaning of 'Eiseniella tetraedra'. Suppose that, in a practical test to see if you understand this expression, your examiner gives you a tray of several specimens and asks you to say which if any belong to the species. You might happen to know that the only available specimen of this species was kept in box 57, and have seen through the keyhole that the specimen on the left of the tray came from that box. Given that you would know which of the test-specimens belonged to the species. And by saying 'All individuals serially very like the specimen on the left' you could precisely mark off the set of members of the species. But in this situation you could not be said to understand the species' name. This would be cheating. You understand only if you can at least sometimes correctly classify a given specimen on the basis of features observable in the specimen itself—such things as being yellowish and one inch long, having four equally-spaced bristles on each body-segment, and so on. You must be able to tell *what sorts* of individuals belong, not merely *which* individuals.

Suppose that you minutely examine and describe a single specimen of the species, listing the attributes you find in it. The conjunction of this large set of attributes defines a certain class. If you have done your work well the specimen before you will be a member of it, and there will perhaps be other organisms that exactly fit your description and so belong to the class. Let us call any class which, like this, is non-empty and is defined by a conjunction of many attributes a 'form'. (The word is sometimes used in roughly this sense by taxonomists.)

The form is not the species. Suppose that you examine and describe a second individual, and find it to be very slightly different from the first. Since the difference is very slight, it too is a member of the species. But since there is a difference it will not quite fit the detailed description previously given. It will not belong to the form just defined. A detailed description of the second specimen, though very like the first description, will diverge very slightly. This second description will therefore define a distinct form, though one very very like the first one. Any further individual encountered, if it differs only very slightly from some individual already ranked as a species-member, will also be a member; and a detailed description of it will define yet another form, very like some form already recognised as within the species. Plainly the different forms that make up the species are a chain-cluster integrated by *serially very like*.

You can identify a cluster only if you can identify some member of it, and know the integrating relation. To show that you understand a species-name you must be able to identify some specimen as a member of it. But it will not do, as we saw, if you can merely tell that a specimen is a member *qua* particular individual—this one rather than that one. You must identify it by its observable attributes—as of this sort rather than that sort. You must, in short, recognise the form of your specimen as being such as to qualify it for admission into the species. Hence a morphological species must, strictly speaking, be regarded as a chain-cluster of forms, not one of individuals.

Very similar considerations apply to a biological species. It, too, must be regarded as a chain-cluster of forms. But here the integrating relation is *serially crossable with*.

Considered (wrongly) as a chain-cluster of individuals, a species is highly unstable. Old members are constantly dying and decaying, and so leaving the cluster. New members are as constantly being added, so that every few months or years there is a complete turnover of membership. But, considered as chain-clusters of forms, most species are remarkably stable. Occasionally a new form comes into existence, very like some existing form within the species. A new member is added to the cluster. Occasionally a form dies out: the cluster loses a member. But these changes are slight: the adding or subtracting of one to a cluster of very many forms. A single cluster sometimes splits in two, of course. Two new species evolve out of a single ancestral stock. But, except in the case of micro-organisms, where the whole thing is vastly speeded up, such fissions happen so slowly as to be imperceptible during a human life-time.

All the points we have made about clusters in general apply in the case of species. No form is essentially a member of a form-cluster, or essentially a non-member. It is still the same species after one of the forms has died out: still the same species after a mutation has added one more form to the existing large cluster.

You could be said to understand the species-name provided that (1) you are familiar enough with certain forms—A, B and C, let us say—to be able to recognise individuals as belonging to them, (2) you think that the species-name refers to a cluster of forms integrated by *serially very like*, and that A, B and C are members of it, and (3) this conception is at least mostly correct. My conception (or your conception at a later date) might be of a cluster comprising, among others unknown, C, D, E and F. Yet these different conceptions could, if all six forms were serially very like, both be conceptions of the same species. Despite these fluctuations we might both of us constantly give the same meaning to the species-name.

Empirical investigation in the field, not just observation of word-usage, can show that your conception of a species is to be enlarged. A new form may be found and seen to be serially very like the other forms of the species, so that it is added to the forms which make up your conception. And you can discover that your conception is wrong at one point, that a certain form must be omitted from it. Your original conception might be of the species as comprising, among other forms, A, B, C, D, E, F and M; the last being a form rather different from the others, which you thought to be linked with them by a continuous series of forms unknown to you. Field investigation might show that such a gap-bridging series did not, in fact, exist. Your conception would then have to be revised by the omission from it of M. Such revisions would be described by saying: 'I discovered a new form (a new "variety" or "variant") of *Eiseniella tetraedra*' or 'Investigations have shown that such-and-such a form has been wrongly assigned to *Eiseniella tetraedra*'. It would not occur to any biologist in such a case to say that the term 'Eiseniella tetraedra' now had a meaning different from that which it used to have.

Thus a species has no nominal essence. It is not we who set up the standards, freely deciding which forms shall and which shall not qualify for admission. Nature takes a hand. We have to discover, by examining organisms, which forms belong and which do not. And we can make mistakes.

The species is surely not something over and above the various forms that make it up. But if any definition, in terms of one or more of these forms, misses the essence and states something merely contingent, what then *is* the meaning of the species name? It is not surely an unmeaning mark! Nor is it a term of wildly fluctuating meaning.[10]

The forms that cluster into species are natural; they are not in the anthropologist's sense 'cultural'. But 'cultural traits'—forms of human behaviour and forms of artefact—can also form clusters, e.g. the different activities which, as Wittgenstein pointed out,[11] make up what we call 'games'. The logic of such 'cultural kinds' has obviously certain features in common with that of morphological species and other natural kinds. But there are, of course, differences.

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[10] On kinds as clusters of classes cf. Whewell, *Philosophy of the Inductive Sciences* 8-2-10 and especially 13-3-26.

[11] *Op. cit.* §66.

## 19. *Kinds as Clusters of Attributes*

A species can be viewed as a cluster of forms. But it can also be viewed in quite a different way. From this other point of view, too, there is a cluster in the picture, but one of a quite different sort.

In calling any aggregate a 'species' we imply that it is reasonably distinct, reasonably well marked off in nature from other aggregates. A non-member of a morphological species cannot be very like any member of it, for then it, too, would count as a member. (This also holds, perhaps even more strongly, for such chemical natural kinds as iron, silver, sulphur, and so on.)

Let me expand this idea. By a 'peculiarity' of a class I mean an attribute common and peculiar to the class, or a set of attributes severally common and jointly peculiar. Thus 'A is a peculiarity of the kind K' means 'a thing has the attribute or conjunction of attributes A if and only if it belongs to K'. Now a peculiarity is what distinguishes a class from other classes, and a class that is notably distinct and well-marked-off from all other classes will be a class that has not one but many peculiarities. So if K is a well-marked-off natural kind it will have not one but many peculiarities, say A, B, C, D and E. Now it follows from what has been said that if anything has any one of these five peculiarities it belongs to K, and that every member of K has all these five. So we can say, dropping out any mention of K: If anything has any one of A, B, C, D and E it has all the rest. To put it another way: this set of attributes or attribute-conjunctions forms a perfect syndrome—every member of the set has to every other member the symmetrical and simply transitive relation *invariable concomitant of*. The set of all peculiarities of a kind, therefore, constitutes a simple cluster integrated by this relation.

So to kinds considered in this way, too, the same general points apply. Instead of defining a species as comprising such-and-such forms, and others serially very like them, one could (and often does) define it by a peculiarity or 'diagnostic'. One simply selects an attribute or set of attributes common and (jointly) peculiar to the member. There are many different but equivalent definitions of this sort: as we have seen, the peculiarities of a kind form a cluster. But no member of the cluster is essentially a member. We could discover that E, say, was not, as we had supposed, a concomitant of the remaining members of the syndrome; or that F, unnoticed previously, should be added to the list. Hence any definition by peculiarity is open to correction in the light of further empirical investigation. Yet such corrections, and differences of definition from person to person, would not entail that there was any fluctuation in the meaning of the species-name. And so on.[12]

## 20. *States and Properties as Clusters*

Consider the property of 'being at a temperature of 290 degrees Centigrade'. There are many different ways of measuring temperature, and thus many different possible 'indications' that a body is at that temperature. Indication A will be 'such-and-such a test procedure would give such a result or reading': indication B will be 'such-and-such another procedure would give such-and-such a result'. Suppose that on our present information there are five such indications of a temperature of 270°, namely A, B, C, D and E. Since they are all indications of the same temperature they are, on our assumptions, concomitants of each other: a body that gives one of these indications gives all the rest.[13]

A new technique of temperature-measurement might subsequently be discovered. This would mean that there was found to be yet another test-procedure-and-result, F, concomitant with A, B, C, D and E, and so also indicative of a temperature of 270°. And a procedure previously thought to give quite reliable measures of temperature might later be found to give results inconsistent with the unanimous verdicts of other measurement-procedures. So indication A might be dropped from the list of reliable indications of 270°. No indication is essentially a member of the set of indications of 270°, and none is essentially a non-member. The indications form a cluster, with all that that entails. There is no archetypal definition of temperature; there is a number of definitions that happen to be equally sound (though all are open to empirical correction) and you can switch from one to another without any change in the meaning of '270° C'.

Diseases were first apprehended historically as somewhat loose syndromes of such symptoms as headache, rash, vomiting, and so on. And where there is a syndrome, there we have the foundation for a cluster-concept. In modern medicine, however, there is a strong tendency to re-define diseases in terms of causes rather than symptoms.

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[12] On kinds as clusters of attributes cf. Whewell, *op. cit.*, 8-1-10; 8-4-6; 8-4-7. cf. also Campbell, *Physics, the Elements*, Ch. 2.

[13] It would perhaps be more correct to say that the five form an accidental cluster that happens to be integrated by *near-concomitant of*, rather than an essential cluster necessarily integrated by *concomitant of*. But this will not affect the points I have to make.

## 21. *Objective and Subjective*

A case could be made out for saying that an objective event, such as the occurrence of a real objective smell at a certain locality, is a cluster of subjective events.

One possible indication that there is a real smell at locality L is the fact that Smith, if he were at L and did not think he was subject to an olfactory delusion, would be prepared sincerely to claim that he could smell a smell. Another possible indication is the fact that Jones, under a like proviso, would be similarly prepared. Or, to put it more shortly: one indication of a real smell at L is that there is a smell at L to Smith; another is that there is a smell at L to Jones. Now it is an empirical fact that when there is a smell at a given place to one person there is *nearly* always also a smell there to *nearly* everyone else. But for this fact we should not have the concept of an objective smell.

We can make a spatial model of this as follows: The twenty-six ships, A to Z, are all the ships there are. The big majority of these are, at any given moment, to be found clustered together. There are usually one or two stragglers far away from this main body, but it is not always the same handful that is away. Any of the ships could, at any moment, be one of the stragglers. But most of the ships are most of the time bunched with most of the others.

Then, given that A is off Naples, we have good empirical grounds for thinking that most of the rest of the ships—the big cluster of them—is off Naples too. And given that the big cluster is off Naples we have empirical grounds for thinking that A is there. There are no entailments, either way, for A could be one of the stragglers. But there are high probabilities, for on any given occasion any given ship is unlikely to be a straggler. So given the conjunction 'A, B and C are all off Naples' you have excellent grounds for concluding that the big cluster is. With bigger and bigger conjunctions you get nearer and nearer to a premiss that entails that the cluster is there. There is no way of confirming the presence of the cluster more direct than that of observing more and more ships: there is no seeing of the cluster apart from the ships.

Compare: Given that there is a smell here to A we have good empirical grounds for thinking there is a real objective smell here. And given that there really is a smell here we have grounds for thinking that there will be a smell here to A. But there are no entailments, for A could have a bad cold, or be subject to olfactory illusion. There are high probabilities, however, for olfactory illusions and really bad colds are rare. Given that there is a smell here to A, B and C, you have excellent grounds for thinking there is a real objective smell. Given that there is a smell here to hundreds of people, you have a premiss that pretty well entails that there is an objective smell here. There is no way of confirming that there is an objective smell here more direct than of smelling it yourself and getting reports from more and more people that there is a smell here to them too. This is the kernel of truth in phenomenalism.

### 22. *Semantically Ultimate Clusters*

Of the clusters described, two stand out as being 'semantically ultimate', namely the enduring material body considered as a cluster of event-like 'segments', and the objective event considered as a cluster of subjective ones.

In every other case it is possible to imagine a language in which there are expressions for the items but no words for clusters of them, and possible to imagine cluster-words being introduced and explained to a people who as yet had only item-words. But not in the above two cases.

How can you speak of a particular episode, a particular constitutive segment of a space-time worm? By a phrase of the form: 'At such a place and time a body exhibiting such-and-such features'? But such a phrase, it could be urged, contains the word 'body', and so it cannot be a phrase in the language of people who have only item-words and no cluster-words. This point could perhaps be met by saying that in such phrases 'body' was to be construed as meaning something instantaneous, and not an enduring body of the ordinary sort. Or it could be met (this is the most popular version) by saying that the event-like items were compresences of attributes in places at times, and that these could be spoken of in phrases of the form 'f and g at $P_1$ at $t_1$—where 'f' and 'g' designated properties or attributes, $P_1$ designated a place, and $t_1$ a time.

In either case, however, the phrase describing the event will still contain an expression designating some particular point or region in space. Now could people learn a language which contained no expressions for enduring material bodies, but did contain designations of points or regions in space? Strawson[14] has advanced powerful arguments to show that this is impossible—that place-designations and enduring-body-designations get into the language together, and that it would be self-contradictory to suppose that a person could learn to use one of these two sorts of expression without learning to use expressions of the other sort. If these arguments are sound then, even if we are right in viewing bodies as clusters of events, bodies are semantically ultimate in the sense that there could not be a pure event-language.

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[14] *Individuals* Ch. I. See esp. p. 37.

Likewise there are powerful arguments[15] to show that people could not learn a language which contained only sentences of the type '*To me* there is a smell here', and contained none that were used in the manner of 'There *is* a smell here'. Suppose that you already have a fair mastery of 'There is a smell here', so that with but few exceptions you are prepared to say this whenever and only when the bystanders, if any, are prepared to say it. Then we can teach you the use of 'to me there's a smell'. We must wait for one of the exceptions—to an occasion on which you say 'There's a smell here' although none of the rest of us are prepared to say this. On such an occasion we can say to you: 'No, there isn't really a smell here—it's merely that *to you* there's a smell'. But until such an occasion arises you cannot be taught the use of any expression of the 'to me' sort. Hence there could not be a language consisting solely of such expressions. 'To me there is' semantically presupposes 'there is'.

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[15] Wittgenstein's discussion of 'private languages' is relevant here. I owe the particular argument I use to W. Sellars' paper in *Minnesota Studies in the Philosophy of Science, Vol. I.*

# SOME FUNDAMENTAL PROBLEMS OF DIRECT MEASUREMENT

By Brian Ellis

Measurement is the means by which mathematics is applied to the study of physical phenomena. And as such it is extremely important that we should study its nature carefully.

In this paper, I wish to consider some questions which I think are fundamental. The first group of such questions concerns the nature of quantity. What sort of thing is a quantity, and how can we decide when a quantity exists? The second group concerns the nature of fundamental measurement. By 'fundamental measurement' I understand a form of measurement which does not depend upon prior measurement. Fundamental measurement therefore lies at the very heart of all measurement. But under what conditions is a quantity fundamentally measurable? And how arbitrary are our conventions for assigning numbers to things by means of fundamental measurement?

## I. *The Nature of Quantity*

I think it will be agreed that a measurement must yield the measure of some quantity. But under what conditions can a thing be said to have or possess a quantity? It can be said at once, I suppose, that if some things have a quantity in common, whether it be length, area, mass, pressure, charge or temperature, then it must make sense to compare these things in respect of this quantity, and say whether one is greater than, equal to, or less than another in this respect. And conversely, I think it will be agreed, if we can always decide whether one thing is greater than, equal to, or less than another in some respect, then the respect in which these things may be said to be equal or unequal may be described as a quantity. At any rate, this is the way in which I propose to use the term 'quantity'.

But under what conditions can we say that one thing is greater than, equal to or less than another in some respect? It should first be noted that there are different kinds of equality. Sometimes it makes sense to say that two things are equal to one another in some respect when it makes no sense to say that one is greater or less than another in this respect. For example, we can say that two things are equal or the same in colour or shape, but we cannot say that one thing is greater or less than another in colour or shape. An equality of this type may be described as a *qualitative equality*.

The kind of equality which is of interest to us here is not of this type. For if two things can be said to be equal in respect of some quantity, it must also make sense to say that one is greater or less than another in this respect. And it is with this kind of equality, namely quantitative equality, that we shall be concerned.

Every relationship of quantitative equality must be symmetrical and transitive. But not every symmetrical and transitive relationship is a quantitative equality. For relationships of qualitative equality are also symmetrical and transitive. Every relationship of quantitative inequality is necessarily asymmetrical and transitive. But not every asymmetrical and transitive relationship is a quantitative inequality. The relationship of causality is asymmetrical and transitive, but it is not a 'greater than' or 'less than' relationship.

For any given quantity p, there must be three possible relationships of quantitative equality or inequality—a 'greater in p than', an 'equal in p to' and a 'less in p than' relationship, the first and third being converse relationships. Together these three relationships must form a group of mutually exclusive alternatives. That is, if one in fact holds, neither of the others can in fact hold. But if one holds, it must make sense to say that either of the others holds.

The following, then, are conditions which must be satisfied before we can say that one thing is greater than, equal to or less than another in some respect. The things concerned must be connected by one or other of a group of relationships $R_+$, $R_0$ and $R_-$,

(1) $R_0$ being a symmetrical and transitive relationship.

(2) $R_+$ and $R_-$ being asymmetrical and transitive converse relationships.

(3) $R_0$, $R_+$ and $R_-$ being mutually exclusive alternatives.

And the existence of such relationships is thus a necessary condition for saying that the things concerned have or possess a quantity in common.

The question now arises whether this is also a sufficient condition for saying that two things possess a quantity in common. All that one can say here is that this seems to accord fairly well with ordinary usage. And I, therefore, propose to make it a matter of definition that a quantity exists when such relationships exist. But, of course, it can still be asked whether this is a useful way of speaking.

It is not difficult to show that if the members of a class of things are connected by such relationships, then, provided that these relationships are stable and determinable, these things can be placed in an order from which we can "read off" their relationship to one another. Things on the same level will be connected by an $R_0$ relationship, and things which occur in the order will be connected by an $R_+$ relationship to those below them. Relationships having the properties of $R_+$, $R_0$ and $R_-$ may therefore be described as *ordering relationships*.

Now the suggestion I wish to make is that wherever such ordering relationships exist, we should say that a quantity exists. And that whenever we have succeeded in ordering a group of things by means of such relationships, we should say that we have placed them in the order of some quantity. And I would further suggest that the discovery of a new quantity is the same thing as discovery of a new group of ordering relationships.

Let me illustrate this with the following two examples:—

*Example 1*

The various states of a freely swinging pendulum are connected by three possible, but mutually exclusive, transformation relationships, having the properties of $R_+$, $R_0$ and $R_-$. They are:

(1) A state A may transform itself into a state B and conversely when the pendulum is subject to no damping, but not when it is subject to damping.

(2) A state A may transform itself into a state B, with suitable damping, but not conversely, or in the absence of damping.

(3) A state B may transform itself into a state A with suitable damping, but not conversely, or in the absence of damping.

These relationships have all of the required characteristics for ordering relationships. They are mutually exclusive and alternative. The second and third are the converse of one another. The first is symmetrical and transitive, and the second and third are each asymmetrical and transitive.

Now in fact we say that the various possible states of a pendulum have a certain quantity to different degrees, and we give it a name—'mechanical energy'. If two states A and B of a pendulum are related by (1), we say that they are equal in mechanical energy. And if they are related by (2), we say that A is greater in mechanical energy than B.

These, of course, are not general ordering relationships for the quantity we call 'mechanical energy'. Other things besides the states of pendulums possess mechanical energy. But they are ordering relationships for the quantity we call 'the mechanical energy of a state of a pendulum'.

*Example 2*

The various possible states of an energetically isolated system may be connected by one or other of the following transformation relationships:

(1) A is transformable reversibly into B—a symmetrical and transitive relationship.

(2) A is transformable irreversibly into B—an asymmetrical and transitive relationship.

(3) A is not transformable either reversibly or irreversibly into B, but B is transformable irreversibly into A—an asymmetrical and transitive relationship which is the converse of (2).

The three relationships are mutually exclusive alternatives, and they have all of the required characteristics for ordering relationships. Accordingly we say that the various states of an energetically isolated system have a certain quantity to different degrees. In this case the quantity is called 'entropy'.

In contrast to the previous case, these ordering relationships are general ordering relationships for entropy. Things can be compared in respect of entropy if and only if such ordering relationships connect them. And that there are relationships of the second and third kinds is the most general form of the Second Law of Thermodynamics. This is the form in which it was expressed by Carathéodory.[1]

I have taken these two cases because I think that they are always troublesome to students of the physical sciences. The student asks what mechanical energy or entropy is, and he is given a formula—a means of calculating the measure of it from other measurements. Or else he is given a theoretical interpretation of it, when he doesn't yet know what it is he is supposed to be interpreting. He wants to know what he is measuring or interpreting. He wants to be shown some property, some feature of the system itself, which is the mechanical energy of a pendulum, or which is the entropy of the state of a system. If I am right, the students' demands are based on a misconception of the notion of quantity. To point to a quantity is to do no more than to point to a group of ordering relationships.

The second of these examples also illustrates my thesis that the discovery of a new quantity is the same thing as the discovery of a new group of ordering relationships. For it was the realization that the various possible states of an energetically isolated system may be connected by irreversible, as well as by reversible, transformation relationships, which led Clausius to the concept of entropy.

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[1] See Buchdahl, H. A., 'On the Principle of Carathéodory', *Am. J. of Phys.*, Vol. 17, No. 1, pp. 41-43, January, 1949.

It should be noted that so far I have done no more than try to state necessary and sufficient conditions for saying that a quantity exists. These conditions must not be confused with criteria for the identity of quantities. I am not saying that for each quantity there must exist one and only one set of ordering relationships. I can see no reason why there should not be any number of ordering relationships for a given quantity. For example, objects can be placed in order of mass or of temperature by any of several groups of ordering relationships. And it is the *order* which is important for the identity of quantities. If two logically independent groups of ordering relationships lead to the establishment of the same order (or at any rate, nearly the same order) among the same things, then we should say that they are both ordering relationships for the same quantity. And this, I think, is both a necessary and sufficient condition for saying this.

It should also be noted that if my analysis of quantity is correct, then the discovery and identification of quantities is logically independent of the discovery of a means of measurement. And it is therefore reasonable to ask: what makes a quantity measurable? The mere existence of an order does not in itself imply measurability in any important sense of this word. We could, of course, arbitrarily correlate numbers with levels, as in Mohs' scale of hardness, provided that the order is stable. But we do not feel that this is really measurement. We feel that the numbers we assign to things, when we make measurements, must do more than indicate an order.

## II. *Fundamental Measurement*

All measurement ultimately depends upon certain basic forms of measurement. The measurement of temperature, for example, may depend upon the measurement of pressure, and the measurement of pressure may depend upon the measurement of length. But the measurement of length does not, or need not, depend upon anything else. It is thus a basic form of measurement. But it is not the only basic form of measurement. The measurement of mass, time interval, electrical resistance, electrical potential, area, volume, for example, does not, or need not, depend upon prior measurement. We could measure these quantities even if we could measure nothing else. Not all quantities, however, are thus directly measurable. There are many quantities whose measurement always does, and must as far as we know, depend upon prior measurement. They are only indirectly measurable. Temperature is such a quantity. So is pressure. And so are energy, entropy, quantity of heat, and density.

The procedures for measuring the basic quantities are strikingly similar to one another. Each depends upon the adoption of a criterion for equality and the matching of objects by a series of repeated operations with a standard object. In each case, measurement involves counting the number of such operations required for the production of equality. So similar are these procedures, indeed, that it is possible to give a general description of them, which applies to them all, and which leaves out nothing of significance. I am not sure that this description must fit all direct measuring procedures for basic quantities. But at least it seems to me to fit a large and important class of them. Accordingly I propose to classify such procedures as fundamental measuring procedures, and to call the kind of measurement which results from their employment, 'fundamental measurement'.

All fundamental measurement depends upon the existence of a procedure for producing equality in respect of the fundamentally measurable quantity.[2] In the fundamental measurement of length, the object to be measured is equalled in length by a line produced by a series of repeated operations with a standard or set of standard objects (i.e. measuring rods). In the fundamental measurement of mass an object (or group of objects) equal in mass to the mass to be determined is produced by a series of operations with a set of standard masses. In the fundamental measurement of electrical resistance, the object whose resistance is to be measured is matched in this respect by an object produced by a series of operations with a standard resistance or set of standard resistances. (This is the Wheatestone Bridge method.)

In all fundamental measurement there is a direct criterion for equality. In the case of length measurement, two things, A and B, are said to be equal in length if A covers B without overlap, and B covers A without overlap. In the case of mass measurement, two things A and B may be said to be equal in mass, if they can replace each other on a scale pan without tilting effects.

In each case, we can use this criterion to build up a set of objects equal to any chosen object in respect of the fundamentally measurable quantity. Let us call any such object an object $S_1$.

In all fundamental measurement, two objects $S_1$ can be combined to form a composite object $S_2$, which is greater than any $S_1$ in the fundamentally measurable quantity. And the procedure is such that all objects $S_2$ thus produced are equal to one another in the given respect. Further, by continuing with the same procedure, three objects $S_1$ can be combined to form a composite object $S_3$, which is greater than any $S_2$—all such objects $S_3$ being equal in the fundamental quantity. And, generally, $n+1$ objects $S_1$ can be combined by the same procedure to form a composite object $S_{n+1}$, the objects $S_{n+1}$ thus produced being equal to one another and greater than any $S_n$. In this way the fundamental measuring procedure can be used to build up a set of standards, $S_1$ $S_2$ $S_3$ . . . increasing in order in the fundamental quantity. Call any such set, a standard set.

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[2] A fundamentally measurable quantity is not necessarily a fundamental magnitude from the point of view of the theory of dimensions. Area is fundamentally measurable (with measuring areas instead of measuring rods) but it is not a fundamental magnitude.

Similarly, in all fundamental measurement, a sub-set can be formed. Two objects $S_{\frac{1}{2}}$, less than $S_1$, and equal to one another in the given quantity, can be found such that when they are combined by the fundamental procedure, they form a compound object $S_1$, where all such objects $S_{\frac{1}{2}}$ are equal to one another in the given respect. Further, three objects $S_{\frac{1}{3}}$, four objects $S_{\frac{1}{4}}$, and so on, can be found. And a sub-set of standards $S_1$, $S_{\frac{1}{2}}$, $S_{\frac{1}{3}}$, $S_{\frac{1}{4}}$ . . ., decreasing in the fundamentally measurable quantity, can be formed. Call any such set a standard sub-set.

In all fundamental measurement, compound objects formed by the fundamental measuring procedure from members of the standard set or sub-set are equal to one another in respect of the given quantity if and only if the arithmetical sum of the indices of the standards used in their production is the same.

And finally, a fundamental measurement is made by matching the object to be measured with a compound object formed by the measuring procedure from members of a standard set or sub-set.

This description of fundamental measurement applies to all of the fundamental quantities which I have mentioned. And as far as I can see, it leaves out nothing of importance. If I am right in this, then any procedure for measuring a quantity which accords with this description could properly be called a fundamental measuring procedure.

In giving this description, I have tried to avoid all reference to our conventions for assigning numbers to the members of standard sets or sub-sets or to composite objects produced from them. Conventionally we assign the number 1 to an object $S_1$, the number 2 to an object $S_2$, and generally the number n to an object $S_n$, where n is any rational number. But this, as I hope to show, is a matter of convention. And it is not essential to the description of fundamental measurement. If we include this convention in our description of fundamental measurement, then we immediately get the result that the fundamental magnitudes must be additive. That is, there must exist a physical interpretation for the arithmetical operation of adding length or mass or volume numbers. But I cannot see that additivity is essential to fundamental measurement—although this is often claimed to be the case.

We have already noted that in general there may be any number of criteria for equality or inequality for a given quantity. This, of course, applies to the fundamental quantities, too. With mass, for example, we could use a balancing test rather than a substitution test for equality. And the results of this test would be logically independent of the results of the former one—although in fact, I believe, always the same. Thus we could use the following set of ordering relationships for mass by setting up a beam balance with scale pans L and R:—

(1) An object A could be said to be equal in mass to an object B if and only if A on L balances B on R and A on R balances B on L.

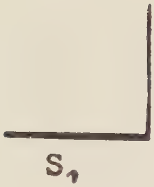
(2) An object A could be said to be greater in mass than an object B if and only if, with A on L and B on R, A descends, and with A on R and B on L, A descends.

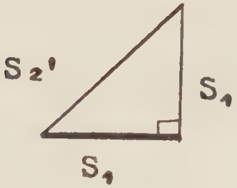
(3) An object A could be said to be less in mass than an object B if and only if, with A on L and B on R, A ascends, and with A on R and B on L, A ascends.

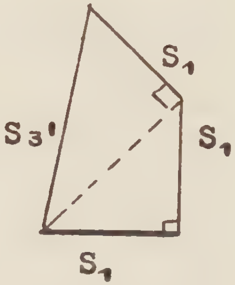
But even when we adopt the same criteria for equality or inequality for a fundamental magnitude—so that there can be no doubt whatever that we are dealing with the same quantity—there may yet exist any number of different fundamental measuring procedures. And it is not difficult to show that if we begin with the same standard, and adopt the usual conventions for assigning numbers to the members of the standard sets and sub-sets, then, depending on which procedure we follow, we will be led to assign different numbers to the same objects as the measure of the same quantity.

Let me illustrate this point by showing how a radically different scale of length can be set up by following a rather eccentric fundamental length-measuring procedure.

The normal measuring procedure for length is to place objects end to end. And the set of standard lengths $S_1$, $S_2$, $S_3$ . . . is produced by doing this with objects equal in length. If, however, we start with objects equal in length, and put them together in the following way:







and so on, we will be able to construct a set of objects $S_1$ $S_2'$ $S_3'$ . . . having all of the characteristics required of a standard set of lengths. For it can be readily verified geometrically that any compound objects formed by this procedure from the members of this set will be equal in length if and only if the arithmetical sum of the indices of the objects used is the same.

This point can be brought out most easily by showing how this 'right-angled addition' procedure might be used to set up a scale of length, differing from the ordinary length scale, but having all of its important logical characteristics. To do this, let us adopt the ordinary convention for assigning numbers to the members of a standard set.

Select some object as a standard, and let its length be 1 diagonal inch—a 'dinch', for short. And suppose that

$$1 \text{ dinch} = 1 \text{ inch}.$$

If now we combined two standard dinches by the right-angled addition procedure, we will obtain the second member of our standard set. And according to the usual practice in fundamental measurement, we should say that its length is 2 dinches. But a line of length 2 dinches is the diagonal of a square of sides 1 dinch. Accordingly

$$2 \text{ dinches} = \sqrt{2} \text{ inches}.$$

The third member of the standard set, a line of length 3 dinches, may be obtained by combining a line of length 2 dinches with a line of unit length by the same right-angled addition procedure. And, of course,

$$3 \text{ dinches} = \sqrt{3} \text{ inches}.$$

In general it will be seen that

$$p \text{ dinches} = \sqrt{p} \text{ inches}.$$

It can also be seen that dinches are additive by the right-angled addition procedure. If a line of length p dinches is 'added to' a line of length q dinches, a line of length $p + q$ dinches will be obtained. This follows immediately from the fact that the diagonal of a rectangle of sides $\sqrt{p}$ and $\sqrt{q}$ inches is $\sqrt{p + q}$ inches.

Unless I have omitted some vital factor from my description of fundamental measurement, it follows from this that there may in general be more than one fundamental measuring procedure for a given fundamental magnitude. And this at once raises an interesting question: Why do we use the fundamental measuring procedures that we do? The obvious answer that no one has ever suspected that there were such alternatives does not satisfy me. For this makes it look like an historical accident that we use the kind of length scale that we do. And I find this difficult to believe. In the case we have discussed, there can be no doubt that the ordinary length-measuring procedure is easier to follow. But can this be the only reason? If it is, then it certainly makes our length scale look very arbitrary. Grounds of convenience are not usually regarded as being of much significance. The mercury scale of temperature is easier to set up than the thermodynamic scale. But grounds of convenience are completely over-ridden by other grounds. However, if it is not a matter of convenience, what is it? What justifies our feeling that the kind of scale that we use is the only natural one, and that any other scale is forced and unnatural?

It may be, partly, that from childhood we have learned to estimate distances by procedures very like those used in ordinary fundamental length measurement. We estimate distances by counting the number of steps necessary to get from one place to another, by the number of telegraph poles, by the number of city blocks, and so on. And the whole of our thinking about distance must to some extent be coloured by our upbringing. But this is only to explain our feeling, not to justify it.

Perhaps a satisfactory solution of this problem is to be found by considering the role of distance variables in physical theory. If we change our scale to the diagonal scale, we will have to say that a body moving without friction on a smooth horizontal plane covers a distance proportional to the square of the time interval—unless, of course, we also change our time scale. We will have to say that a freely falling body covers a distance proportional to the fourth power of the time interval. And, in general, wherever r (the ordinary distance) appears as a variable in a physical equation, we will have to substitute $\sqrt{s}$ (where s is the diagonal distance). And it may be argued that this would only serve to complicate our physics.

But I am not entirely convinced by this argument. I have not seen it worked out in detail. And, in any case, there may, for all I know, be some procedure, resulting in some substitution for r (the ordinary distance), which would in fact simplify our physics.

It is, however, important to recognize the existence of this problem—the problem of choice of fundamental measuring procedure. It may not be a practical problem, but a solution of it should help us to a better understanding of the nature of fundamental measurement. And it is surely important that we should try to understand fully the basic means by which mathematics is applied to the study of physical phenomena.

The existence of this problem points also to the existence of another problem—a problem of choice of principle of correlation. If a change of fundamental measuring procedure can change our scale, why should we not simply change our scale? Why bother to look for a fundamental measuring procedure which will make the new scale additive? Why, after all, should we assign the number 2 to the second member of a standard set? Why not $2^2$ or log 2? Why must our scales of fundamental magnitudes be additive? Why not multiplicative? Perhaps these questions can also be answered by considering the role of a given quantity in physical theory. But it is important, I think, that they should be answered.

Melbourne University.

## DISCUSSION

# ON NOT BEING GULLED BY RAVENS

By W. J. Huggett

According to the Paradox of Confirmation observing a non-black non-raven renders probable the empirical generalization

(A) All ravens are black.

The paradox arises in the following way: it is assumed that observing a black raven confirms (A) and that observing a non-black non-raven confirms

(B) All non-black things are non-ravens.

The "unquestionable" equivalence condition is next noted, viz., if an observation confirms one formulation of a proposition it confirms any logically equivalent formulation. Since (A) and (B) are contrapositives and so logically equivalent, it follows that the observations of non-black non-ravens which confirm (B) will also confirm (A).

Bold measures have been adopted by some in order to circumvent this paradox. J. W. N. Watkins,[1] for example, has simply denied that observations of black ravens confirm "All ravens are black" and that observations of non-black non-ravens confirm "All non-black things are non-ravens". If his position could be maintained the unwanted non-black non-ravens would thereby be eliminated as confirming instances of the generalization "All ravens are black". But Mr. D. Stove, in a recent contribution to this *Journal* (August, 1959), has shown that Watkins has not succeeded in supporting the view that "instantial confirmation cannot accredit or render 'probable' a universal generalization" (p. 149). If the Paradox of Confirmation is to be avoided, therefore, other techniques are required.

One promising method that has not as yet been explored is to deny that (A) and (B) are equivalent, i.e. to deny that the "unquestionable" equivalence condition mentioned by Watkins and Stove holds in the confirming situation. The success or failure of this suggestion will depend upon a satisfactory symbolic interpretation of the propositions (A) and (B).

(1) In previous discussions of the Paradox of Confirmation it has been customary to interpret (A) as

(Aa) For all x, raven x $\supset$ black x,

and (B) as

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[1] 'Between Analytic and Empirical', *Philosophy*, 1957.

(Ba) For all x, non-black x ⊃ non-raven x.

Now, since the horseshoe symbol ( ⊃ ) is expendable, (Aa) lends itself to a more basic interpretation,

(Ab) There are no x's such that raven x and non-black x.

Or, more simply, There are no non-black ravens. And similarly (B) becomes

(Bb) There are no non-black ravens.

On this interpretation of (A) and (B) it is obvious that precisely those observations which confirm (or disconfirm) (A) will confirm (or disconfirm) (B).

The question we must now consider is this: Just what observations *will* confirm (Ab) and (Bb), There are no non-black ravens? The most natural answer is surely, *Not* finding non-black ravens. And the obvious *dis*confirming observations will be *finding* non-black ravens. But it appears that finding either black ravens or black non-ravens or non-black non-ravens will be equally ineffective in confirming that there are no non-black ravens. The only observational means of assuring oneself that something of a certain description does not exist is by not finding anything of that description.

Let us consider by way of illustration a real-life situation in which a proposition of the same form as (Ab) and (Bb) was employed. According to a recent news report a sea-captain of many years' standing expressed disbelief on hearing that the liner *Queen Elizabeth* had been struck by 80-foot waves. His contention was that

(C) "There is no such thing as 80-foot waves on the Atlantic".

And the confirming evidence proffered was that in 40 years on these waters he had "never seen waves higher than 50 feet . . . and", he added, "you don't see very many of those". As one would expect, neither the existence of 50-foot waves, nor of 50-foot trees, nor of 80-foot monuments was given as confirming evidence for (C). The example therefore once more indicates that it is the failure to observe something with the specified characteristics, and this alone, which confirms statements of the form (Ab) and (Bb).

What I take it has hereby been demonstrated is as follows: On the customary interpretation of (A) and (B) as negative existential propositions (Ab) and (Bb), observations of non-black non-ravens do not confirm (A) and (B)—but neither do observations of black ravens. The Paradox of Confirmation does not arise when a universal empirical proposition (A) is treated as a negative existential proposition (Ab).

(2) Some might reject the resolution just proffered because they believe it is based on the highly dubious assumption that (Ab) conveys the same existential import as (A). For those who take this position a more acceptable interpretation of (A) must be found. Clearly (Aa) as it stands will not fill the bill. Since (Aa) states that if anything is a raven it is black, it obviously lacks the existential import of (A). Let us therefore modify (Aa) so that it will carry the same existential import as (A). This can be readily done as follows:

(Ac) If anything is a raven it is black, and there are black ravens. Now our question is: What observations will confirm (Ac)? Observing black ravens will clearly confirm the second conjunct of (Ac) and if we are charitable we may suppose that it will confirm the first conjunct as well. If (Ac) is confirmed in this way, so also is (A), which is equivalent to it.

We will next have to consider whether or not the equivalence condition when applied to the propositions (A) and (B) disturbs this cozy picture by introducing observations of non-black non-ravens as confirming instances of (A). Corresponding to (Ac) is the proposition

(Bc) If anything is non-black it is not a raven, and there are non-black non-ravens.

If we argue once more as in the previous paragraph, it follows that observing non-black non-ravens will confirm (Bc) and also (B) which is equivalent to it. But since (Bc) is patently *not* equivalent to (Ac) it does not follow that observations of non-black non-ravens serve in any way to confirm (A). The Paradox of Confirmation, therefore, does not arise when an empirical generalization is treated as a conjunctive proposition one of whose members is an affirmative existential proposition of the type mentioned.

I have confined my attention to what are perhaps the two most plausible ways in which an empirical generalization can be symbolized, and we have seen that in neither case does the Paradox of Confirmation arise. All other plausible interpretations of such propositions would need to be explored before it could be asserted with complete confidence that the Paradox of Confirmation can always be avoided.

University of Manitoba.

# A REPLY TO MR. WATKINS

## By D. STOVE

Mr. Watkins' reply[1] to my note is as ineffective as its title and tone are insulting.

1. What he tells us in his last paragraph about Popper's measure "*C*" of degree of confirmation is familiar to me and far from peculiar to Popper, but it has no relevance. I happened to be writing about something else, namely the belief that observations genuinely support a hypothesis only if they are the outcome of unsuccessful attempts to falsify it. If that is *not* a belief which Mr. Watkins and Popper hold, I would be glad to hear it.

It was that belief, at any rate, which in his original article Mr. Watkins tried to establish, by using the paradox of the ravens to show the futility of trying to support universal generalizations by mere instantial-confirmation. And it was that belief which, I tried to show, turned the existence of evidence for a hypothesis into a question of the history and psychology of individual scientists, and, more specifically, into a question of their expenditure of *effort* in connection with the hypothesis.

Mr. Watkins replies, with startling irrelevance, that Popper's measure of degree of confirmation in terms of probabilities is an "objective measure",[2] having nothing to do with the history and psychology of individual scientists. This reply happens to be false as well as irrelevant. Popper stresses that his "*C*" is not to be interpreted as measuring the degree of confirmation of a hypothesis "*h*" unless the evidence-statement "*e*" "consists of reports of the outcome of sincere attempts to refute *h*, rather than of attempts to verify *h*".[3] Hence even this supposedly objective measure contains the belief which I criticized, what Mr. Watkins aptly calls[4] a "Labour Theory of Confirmation". But the converse is not true, and I was not discussing Popper's measure of degree of confirmation at all.

2. By a "labour theory of confirmation" is meant the belief, as I put it before, that "there is at least some logical connection between there being evidence for a hypothesis and efforts having been made to test it". As an expression of such a theory, I ascribed to Mr. Watkins the statement that, in order to accredit a hypothesis, evidence "must be observed by deliberate intention and not, so to say, stumbled upon by mere good

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[1] This *Journal,* Vol. 37, No. 3, pp. 240-241.

[2] p. 241.

[3] *The Logic of Scientific Discovery,* p. 414. The same is said on p. 418.

[4] p. 241.

fortune". This ascription was a mistake, for which I apologise. The quoted words were themselves a quotation by Mr. Watkins from another writer. But it must be obvious that this statement is *deducible from* the belief that only unsuccessful attempts to falsify a hypothesis genuinely support it, given only a most trivial additional premise.

We have:

(A) "All genuine confirmations of a hypothesis are observations made in unsuccessful attempts to falsify it."

(B) "All observations made in unsuccessful attempts to falsify a hypothesis are observations made by deliberate intention and not, so to say, stumbled upon by mere good fortune."

(A) and (B) entail (C): "All genuine confirmations of a hypothesis are observations made by deliberate intention and not, so to say, stumbled upon by mere good fortune."

(A) is a belief of Mr. Watkins (and of Popper). (B) is a necessary truth of ordinary English, since an intention-to-falsify must be a deliberate intention.

(C) is the expression of a labour theory of confirmation which I ascribed to Mr. Watkins. So the reader can see for himself how far astray that ascription was!

If Mr. Watkins thinks a labour theory of confirmation is false (as he appears to do, from his repudiating my ascription of (C) to him), he now has no alternative but to abandon (A), the Popperian theory of confirmation.

3. The object of my note was to show that it was only by tacitly employing a labour theory of confirmation that Mr. Watkins made the paradox of the ravens appear to tell against the instantial-confirmation of universal generalizations and in favour of the policy of trying to falsify them. He seems to have missed the point completely, so I will try again to make it plain.

I assume that the theory of confirmation is concerned solely with the relations between theories and observations—relations like "is entailed by", "falsifies", "fails to falsify", etc.; and "observations" means here, with harmless ambiguity, either the observed objects themselves or the "observation-statements" which report these objects. So conceived, the theory of confirmation has nothing to do with the history or psychology of individual scientists. Whether given observations were made with the intention of falsifying a hypothesis, or some other intention, or merely "stumbled upon"—considerations like these I assume to be glaringly irrelevant from the point of view of the theory of confirmation.

Now what does the paradox of the ravens show? It shows that the class of observations (observed objects or observation-statements) which instantially-confirm "All ravens are black" is embarrassingly comprehensive, since anything which is not a raven or is black is a member of it.

But the class of observations (using this word always in the above sense) which instantially-confirm that hypothesis, and the class of observations which fail to falsify that hypothesis, *are one and the same class*. Hence whatever the paradox shows about the former class it shows also about the latter class.

Thus Mr. Watkins, or anyone, could discover an asymmetry, with respect to the paradox, between instantially-confirming "All ravens are black" and failing to falsify it, only by departing from the consideration of observations in the above, objective, sense. This Mr. Watkins did by comparing the class of observations which instantially confirm the hypothesis, not with the class of observations which fail to falsify it, but with the class of observations in which *there have been attempts,* which failed, to falsify it. Now *this* class is obviously the narrower one which we want, containing only ravens; it being logically impossible to *try* to falsify "All ravens are black" by non-ravens, since only where success is not logically impossible can one properly be said to try to do something.

Even so, as I pointed out, this qualification is introduced by Mr. Watkins one-sidedly. For, unlike the class of observations which instantially-confirm "All ravens are black", the class of observations in which *there have been attempts,* which succeeded, to instantially-confirm that hypothesis, is similarly narrow, containing only ravens; it being logically impossible to *try* to instantially-confirm that hypothesis by non-ravens, since only where success is not logically inevitable can one properly be said to try to do something.

But the essential point is that from the point of view of the theory of confirmation any reference to the making of efforts to *acquire* observational evidence is a psychological or historical irrelevance, and that while we stick to the observations themselves and their relations to the hypothesis, the paradox reveals no asymmetry between instantially-confirming a hypothesis and failing to falsify it.

If Mr. Watkins is not committed, by his use of the paradox of the ravens, to attributing evidential importance to human effort, he ought to be able to rewrite the third paragraph of his note omitting all reference to the success or failure of "searches" or "attempts to falsify", i.e. *cashing all such psychological or* *historical expressions in terms of objects observed or observation-statements.* I sincerely urge him to try, and to consider whether what it then says is true.

Sydney University.

## REPLY TO MR. STOVE'S REPLY

### By J. W. N. WATKINS

Mr. Stove says in his 'Reply' that he was not discussing Popper's measure of confirmation at all and that 'it has no relevance'. I find this most perplexing. *I* was discussing Popperian confirmation in the passages he criticised and *his* title was '*Popperian* Confirmation . . .'. Moreover, Popper's measure provides a clear-cut answer to his objection about the allegedly subjective, or psychologistic, character of Popperian confirmation. Perhaps the best way for me to show this is to respond to his very fair challenge to rewrite what I said about the confirmation of 'All ravens are black' omitting psychologistic-sounding phrases like 'search for', 'attempted falsification', etc., and mentioning only hypotheses, observation-statements and syntactical relations between them.

First, let me re-state in a slightly amplified but still very over-simple way the laconic account I gave in my reply of Popperian confirmation. Given a hypothesis $h$, an observation-statement $b$, and background knowledge $k$ ($k$ may be taken to include all accepted scientific theories apart from $h$); and given that $b$ has a certain probability in the presence of $k$ alone—we may write this probability thus: '$p(b,k)$'; then when $h$ is added to $k$ we have three possibilities: (1) the probability of $b$ is increased, i.e. $p(b, k.h) > p(b,k)$; (2) the probability of $b$ is unchanged, i.e. $p(b, k.h) = p(b,k)$; (3) the probability of $b$ is reduced, i.e. $p(b, k.h) < p(b,k)$. According to Popper, in (1) $b$ confirms $h$, in (2) $b$ is neutral to $h$, in (3) $b$ disconfirms $h$. The greatest degree of confirmation of $h$ by $b$ would be where $p(b,k) = 0$ and $p(b, k.h) = 1$. A classic illustration of a single observation giving a high degree of confirmation to a hypothesis is Eddington's eclipse observation in the Gulf of Guinea in 1919. Popper's theory does not make the confirmation this afforded Einstein's theory turn on the *effort* Eddington expended or on any other autobiographical factor. It makes it turn on the impersonal fact that observed star-shift in the vicinity of the sun was quite improbable before Einstein's theory was introduced, but extremely probable afterwards.

Now I take up Mr. Stove's challenge to re-state what I said about confirming 'All ravens are black' in an austerely non-psychologistic terminology. We have a hypothesis $h$ ('All ravens are black'); background knowledge $k$ (which, we assume, says nothing definite about the colouring of ravens, but does say that the number of black things is indefinitely larger than the number of ravens); and four observation-statements: $b_1$ ('This object is no raven'), $b_2$ ('This object is black'), $b_3$ ('This object is a raven and black'), $b_4$ ('This object is a raven and not black'). The introduction of $h$ will not affect the probability of $b_1$ or $b_2$ given $k$, but it will somewhat increase the probability of $b_3$ and strongly diminish that of $b_4$. We shall have: $\mathrm{p}(b_1, k.h) = \mathrm{p}(b_1, k)$; $\mathrm{p}(b_2, k.h) = \mathrm{p}(b_2, k)$; but $\mathrm{p}(b_3, k.h) > \mathrm{p}(b_3, k)$, and $\mathrm{p}(b_4, k.h) = 0 < \mathrm{p}(b_4, k)$.

This simple example is already sufficient to reveal the misconception which underlies the whole of section 3 of Mr. Stove's reply. There he still attributes to me only the single confirming relation, 'fails to falsify' (despite my assuring him in my previous reply that the thesis pinned on me by him—'Observations which fail to falsify $h$ confirm it'—was never held by me and is incompatible with the view of confirmation I do hold); he still argues that Popperian confirmation is just as paradoxical as instantial confirmation since non-ravens and black things fail to falsify and therefore (*sic*) 'confirm' 'All ravens are black', adding once more that the only way I can wriggle out of this is by introducing the subjective intention of *trying* to falsify. A glance at $b_1$, $b_2$ and $b_3$ above will, I trust, finally free him from this major misconception. The relations between $h$ and $b_1$, $b_2$ and $b_3$ are purely syntactical. No subjective intentions have been mentioned. $b_1$, $b_2$ and $b_3$ all fail to falsify $h$. Thus all Mr. Stove's conditions are satisfied. Yet only $b_3$ confirms $h$. $b_1$ and $b_2$ are neutral to $h$. This rebuttal of his main criticism seems conclusive. He holds that Popperian confirmation minus subjective intentions yields the same paradoxical results as does instantial confirmation. I have now proved that it does not: on an instantial theory of confirmation, $b_1$ and $b_2$ are not neutral to $h$ but confirm it since they instantiate it.

To make my point still more abundantly clear I will turn from singular observation-statements to collective observation-reports about all the members of a finite class of observed objects. (Mr. Stove allows that 'observed' is a permissible non-psychologistic term.) Suppose we have $b_5$: 'Of all non-ravens so far observed, some have been black, some non-black', and $b_6$: 'All ravens so far observed have been black'. Clearly, the probability of $b_5$ will not increase when $h$ is introduced—$\mathrm{p}(b_5, k.h) = \mathrm{p}(b_5, k)$. Equally clearly, the probability of $b_6$ will increase when

$h$ is introduced—$p(b_6, k.h) > p(b_6, k)$. Thus on a Popperian theory of confirmation $b_6$ does confirm $h$ whereas $b_5$, while failing to falsify $h$, does not confirm it. On an instantiation-theory of confirmation, $b_5$ does confirm $h$.

I think I have now met Mr. Stove's challenge: I have explained in entirely non-psychologistic terms how 'All ravens are black' is confirmed on a Popperian theory of confirmation, and I have shown that this theory of confirmation *does* avoid the so-called 'paradoxes of confirmation'. This is the main part of my reply. But there is a number of subsidiary questions to clear up. Mr. Stove may ask: 'Why did you express yourself so psychologistically on previous occasions?' Part of my answer is that for my part I do not regard such phrases as 'attempted falsification' as psychologistic. If someone reports that he has attempted, unsuccessfully, to falsify some hypothesis, we do not, as scientists, enquire how tired the effort left him or what sorrow he experienced on not succeeding; we ask him to describe his experiments and to explain their bearing on the hypothesis. The other reason is this. The Popperian theory of confirmation (unlike an instantiation-theory) has practical implications for the conduct of scientific research. The practical import of the theory that, given background knowledge $k$, the degree to which an observation-statement $b$ confirms a hypothesis $h$ varies with the improbability of $b$ given $k$ alone and the probability of $b$ given $k$ and $h$, could be summed up in the following hypothetical imperative: 'If you want to confirm $h$ as highly as possible, look for (or devise) situations which, according to $k$ alone, are most likely to disconfirm or falsify $h$.' And this could be summed up more shortly by saying, 'If you want to confirm $h$, try to falsify it.'

From his references to p. 414 and p. 418 of Popper's *The Logic of Scientific Discovery* I suspect that one thing which misled Mr. Stove into regarding Popper's theory of confirmation as psychologistic is that Popper insists that the idea of a sincere test cannot be altogether formalised. On this I will make two comments. First, his theory, so far as I know, goes further than anyone else's towards formalising it. Second, the fact that there is an unformalisable residue does not imply that this residue is composed of subjective feelings. Despite a tester's subjective sincerity, it might be that his test was not objectively 'sincere' because (though he did not realise this) none of its possible outcomes could have adversely affected the hypothesis he wanted to test. The lack of identity between unformalisability and subjectivity may be illustrated by an analogy. Suppose someone claimed that he had largely formalised the idea of a good move in chess but insisted that this idea has an unformalisable residue.

Would Mr. Stove interpret this to mean that whether a move in chess is good ultimately depends on the private feelings of the individual chess-player — as he does interpret Popper to mean that whether evidence confirms a hypothesis ultimately depends on the history and psychology of individual scientists?

I now consider section 2 of Mr. Stove's 'Reply'. He claims here that I cannot escape his charge of psychologism without abandoning proposition (A) which summarises the Popperian theory of confirmation in these words: 'All genuine confirmations of *h* are observations made in unsuccessful attempts to disconfirm or falsify *h*'. (I have slightly emended his formulation.)

Now we have seen that this theory can also be summarised in a non-psychologistic proposition (A′): 'Given background knowledge *k*, *b* confirms *h* if $p(b, k.h) > p(b, k)$'.

Given that the reference in (A) to 'attempts to disconfirm or falsify' is to be interpreted objectively (*vide* my remarks on 'sincere' tests), then I accept (A) as an adequate summary of the practical implications for scientific research of (A′). But is (A) theoretically equivalent to (A′)? Clearly, any (A)-type confirming observation will also be reportable by an (A′)-type confirming observation-statement: to attempt to disconfirm *h* is to look for an observable situation which is improbable or impossible given *k* and *h*, but probable given *k* alone. But could there be an (A′)-type confirming observation-statement which does *not* report an (A)-type confirming observation? In other words, could there be an observation-report *b* which syntactically confirms a hypothesis *h* but which is reported by someone who was not attempting to falsify *h*—perhaps by someone who had never heard of *h*?

I admit that, logically, this is perfectly possible. There is no illogicality in supposing that a tourist, knowing nothing about Einstein's theory, and finding himself in the Gulf of Guinea during a total eclipse of the sun, might have photographed the eclipse for his own amusement and thus inadvertently have provided confirming evidence for Einstein's theory. I happen to believe that as a matter of fact such accidental confirmation seldom or never occurs in science. Still, I admit that such confirmation is conceivable, that it is taken into account by (A′) but not by (A), and that (A) is therefore a technically inadequate summary of Popperian confirmation. To bring (A) into line with (A′) we should have to amend it thus: 'All genuine confirmations of *h* are observations which (whatever the actual intentions of the observer) *might* have been made in an unsuccessful attempt to disconfirm or falsify *h*'. I am grateful to Mr. Stove for stimulating me to reformulate more adequately this summary statement of Popperian confirmation.

My objection to his attribution to me of a quotation from someone else was chiefly that I had used it in a quite different connection. I was not saying, as he suggests, that a confirmable hypothesis is not confirmed if confirming evidence for it has, as a matter of fact, only been 'stumbled upon by mere good fortune'. I was arguing (whether rightly or wrongly does not concern us here) that there are factual hypotheses which are in principle unscientific in that evidence for them never *could* be deliberately collected but could at best be stumbled upon by mere good fortune. To put it non-psychologistically: there are factual hypotheses which do not raise the probability of any observation-statement.

What are the results of this controversy? I have conceded that I should have spoken of confirming observations which *might* have (rather than have in fact) been made in an unsuccessful attempt to disconfirm a hypothesis. What must Mr. Stove concede? So far as I can see, everything. His original attack contained three charges. In para. (b) he claimed that Popperian confirmation no more avoids the paradoxes of confirmation than does an instantial theory—unless, he added in (c), subjective *effort* is introduced; and in (d) he attacked the idea that effort has any bearing on confirmation. As to (b) I have proved that his claim is wrong; as to (c) I have shown that the Popperian theory of confirmation is not psychologistic; thus (d) is irrelevant.

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# PUTTING BACK THE CLOCK

## I. By J. E. McGECHIE

In a recently published paper ("Laws of Thought", in this *Journal*, May, 1959) Mr. Goddard attempts to describe a universe in which the law of non-contradiction would fail to hold. I propose to discuss some apparently overlooked defects from which his account suffers.

We are to suppose "a universe in which it is possible to 'put back the clock' ", in which "Natural processes repeat themselves in a fixed pattern *which is mirrored* in clocks that work in the following way:

> "Starting at twelve o'clock, say, the minute and hour hands move in the usual way until they record one o'clock. They then continue from half-past twelve and move continuously through to half-past one. They then move from one o'clock to two; then from half-past one to half-past two; and so on.
>
> "This way of recording time *is forced on the inhabitants by the ordinary pattern of events.* A stone which rolls down a hill during the first occurrence of a given half-hour, will again roll down the hill in exactly the same manner during the second occurrence of *that* half-hour. Everything happens twice." (p. 36. My italics.)

(Pedantic objection could be taken to some of the italicised passages but these are not germane to my principal quarrel.)

*Everything happens twice! Everything!* If Mr. Goddard is serious in his description of his hypothetical world, he is presumably serious in using the word 'everything' and means what he says. Everything repeats itself within a periodicity of one hour. He then argues that the *technique* of temporal measurement in his universe would be the same as that in our world were we to choose to use two normal clocks (which I shall for convenience label A and B) one lagging half an hour behind the other, and to *change from reading one to reading the other* at half-hour intervals. But this will not do the trick, for we would read 12.00, 12.15, 12.30 on A, then 12.00, 12.15, 12.30 on B, then 1.00, 1.15, 1.30 on A, then 1.00, 1.15, 1.30 on B, 2.00, 2.15, 2.30 on A and so on. In order to have *clock readings* (*not times*) between 12.30 and 1.00, 1.30 and 2.00, etc., we would need either to read both clocks, and then we might as well use a single ambiguous clock such as that described below, or to stop one clock while using the other. If we chose the former method, we would have two numerically different clock readings for each single event. If we chose the latter, then by reference to our clocking technique no event would occur twice, though there would be incompatible events correlated with a given clock reading.

Let us return to Goddard's universe again. Everything happens twice with a periodicity of one hour. This can be taken in at least two ways—the one Goddard clearly chooses, and another which I shall discuss first. The latter is suggested by his account, which I have just discussed, of how *we* might clock events using two timepieces.

Using the two timepieces A and B, then, we, in *our* universe, could clock events by both (in order to fill in the numerical gaps) and adopt the following criterion for 'happening twice'. If an event E occurs when clock A is reading t, i.e. when clock B is reading t + ½, we are to say that it occurs both at time t *and* at time t + ½. I.e. (thinking of these readings as being derived from a *single* normal clock) the event E occurs 'twice', since 'obviously' t + ½ is later than t.

'E happened twice' then would mean 'E happened when clock A was reading t, clock B was reading t + ½'. Had clock A been identical with clock B (had t and t + ½ been readings of the same clock) we could perhaps say this. Suppose then that they are. It follows that when E happens, clock AB has the reading t as well as the reading t + ½. We need merely use a clock with radially symmetrical minute hand and a double hour hand in the form of a V including an angle of 15°.

Using such a clock we have a choice of saying either 'Everything happens twice' or 'There are two clock readings for every event'. (We could just say 'the clock is ambiguous' and leave it at that.) But if this model is to provide us with the clues to an account of a quasi-Goddardian universe, then supposing ourselves to be watching the unfolding (and refolding?) of events in this world would be like supposing ourselves to be watching two duplicate films of a single sequence of events in our world, projected on to a single screen with a half-hour lag between the films. If the original film is of a normal clock, that clock will seem to have two hour and two minute hands (as described above). If the film is of something which is unchanging, it will appear to be solid (the two images will coincide), if of something changing, parts which move will seem transparent and duplicate. Goddard's stone when moving will be duplicate, both duplicates ghostly, one ⅛th of the way down the hill, the other at the top. The two duplicates will move down the hill, the first reaching the foot in three and a half hours and resting there, the second reaching the foot in four hours and merging with the first to form a solid, normal looking stone.

An analogue for the aural part of our experience would be provided by the sound tracks of the films—the 'bump' when the first duplicate reached the foot would be 'repeated' when the second reached the foot.

In *his* universe, Mr. Goddard argues, "space, or rather movement in space, is discontinuous" (p. 38), but this would not be the case in the model just described, though this would be a case of everything happening twice (or would it?). It seems then that this first description I have given is not the one which Mr. Goddard intends, as may be suggested by the passage from which I quote. Movement must be discontinuous. Were we able to observe the sequence of events the following would be *our* account of the stone's movements, clocked by *our* clocks, not the clocks of the universe we observe:

"A grey stone resting at the top of the hill began at 12 noon to roll slowly down, travelling ⅛th of the way by the end of half an hour (12.30pm). It then vanished, instantaneously reappeared at the top (12.30pm) and rolled as before, after half an hour reaching ⅛th of the way down (1.00pm). Instead of disappearing this time (1.00pm), it continued to roll down for another half-hour until ¼ of the way down (1.30pm), when it again disappeared (1.30pm) reappearing instantaneously ⅛th of the way down moving as it was half an hour before. For the next hour it rolled steadily down reaching the ⅜th mark at 2.30pm, once more disappearing, instantaneously reappearing as before, but ¼ of the way down (2.30pm), then proceeding to roll to the halfway mark in one hour (3.30pm) when it again disappeared, reappearing ⅜th of the way down (3.30pm), rolling to the ⅝th mark by the end of the next hour (4.30pm). . . . Finally, rolling from the ¾ mark (6.30pm) it reached the foot at 7.30pm only to disappear for the last time, reappearing ⅞th of the way down (7.30pm), and then rolled again in half an hour to the foot (8.00pm), where it remained."

It should be apparent that Mr. Goddard's diagram on p. 37 is misleading: the stone has to traverse each part of its path twice; this includes the first and the last ⅛th of its track as well as the intervening stages. Accordingly his first diagram should be amended by adding

| | | |
|---|---|---|
| A | Top | 11.30 |
| A | Top | 12.00 |
| | ⅛ | 12.30 |

to the left-hand side at the top, and

| | | |
|---|---|---|
| | ⅞ | 3.30 |
| N | Bottom | 4.00 |
| N | Bottom | 4.30 |

to the right-hand side at the bottom.

But we have not a description of a world where *everything* happens twice—for the disappearance from the ⅛th mark happens only once, and similarly for each disappearance. Each event of disappearing and reappearing is *unique*. Otherwise here also everything happens twice, reversals taking place every *hour*—according to the outside observer and his clock.

But now suppose the outside observer to clock the motion of the stone with reference to the clock in the tower at the top of the hill. By this clock, at 12.00 the stone leaves the top, at 12.30 it is ⅛th down, at 1.00 p.m. it is ¼ down, . . . at 4.00 p.m. it comes to rest at the foot. By reference to this clock, things only happen once—i.e. at 1.00 p.m. it is ¼ of the way down, etc. (Add 'both times' if you wish.)

Now suppose the viewpoint of an inhabitant of Goddard's universe to be adopted.

At 12.00 o'clock by the tower he sees the stone start to roll (for the second time), at 12.30 by the tower he sees it ⅛th of the way down, at 1.00 by the tower he sees it ¼ of the way down. *Then everything* is as it was one hour before. He 'forgets' ('shudder quotes', since he *cannot forget* what has not yet happened to him) what happened 'before' and 'again' watches the stone rolling from ⅛th of the way down at 12.30 p.m. by the tower, and so on. But *he* remembers when *he* looks back (on *two* subsequent occasions an hour apart) these 'two' experiences as *one*. *He* does not recall the first *and* the second, for there will be no reason for him to distinguish them, no respect, *ex hypothesi*, in which they differ to him. For all *he* can tell, *his* world is exactly like ours—asked, he will say 'Nothing recurs'.

So we could say that Mr. Goddard, setting out to describe a *different* universe, succeeds in describing our own, though it would seem to the outside observer that everything happens twice. But even such an observer could always say 'Really nothing recurs—it is just that I *experience* everything twice'.

The trap lies in Goddard's *using* 'everything' and supposedly *meaning* what he says. We can say 'Really the Goddard universe *is* a different one from ours', but any investigation of ours will fail to find respects in which it differs from Goddard's and vice versa. (It is for this reason that I suggested the first of the alternative models above. There *everything does* happen twice—except duplicatings.)[1] Of course if we are not strict about the 'everything', then there could well be a difference—i.e. if we said, for example, *'everything but* the experiences of inhabitants'—if their experiences were ordered as would be those of the observer of Goddard's universe whose account is given above. Even so, there would have to be gaps in Goddard's account—for, if an observer's experiences did follow this sequence, should we say of him that he can foretell the future, *know* what is going to happen? And how could he differentiate between the first time E occurs and the second, and if so, by reference to what? And if everything, apart from disappearances, does happen twice, though individuals can recall the two occurrences of any given event, which would answer, *for us,* to a given unique description, does it follow that the stone is *both* at the top of the hill *and* ⅛th of the way down at the same time? If the criterion for temporal ordering is *not* to be numerical readings of clocks but the order of the experience of observers, then the stone *can* be said to be both at the top and ⅛th of the way down 'at the same time' and the law of non-contradiction appears to fail. It would appear to fail also when the first account of 'everything happening twice' is accepted. But alternatively in both cases one could say 'Here *reference* is irremediably ambiguous'. On the first account 'the stone' may refer to the transparent body at the top of the hill, or to that ⅛th of the way down, and if it refers to either (but not both) the law stands. If it refers to both the law still stands, for both the law and its statement presuppose the making of unique references.

On the second account, (with the qualifications about 'everything'), reference failure (ambiguity) occurs every hour—'the place where the stone is one hour after it starts rolling from the top of the hill for the second time' is ambiguous. We could equally well choose to say here 'the stone is (every hour on the hour) in two places at once—if it is moving' and we could mark this by choosing to say that the law of non-contradiction fails every hour on the hour (every hour on the half-hour if we take the clocks seriously).

We could, I suppose, drop the requirement of unique reference in our talk, and then 'Mary is both sitting and standing' could be true—two people called Mary, as we would ordinarily say, one sitting and one standing. In fact we 'use' the law, as it were, to *detect* ambiguity of reference. If two people testify to the colour of a certain document in a court of law, one that it is white, the other that it is not, and both testify to having seen it at the same time t, if we have no reason for rejecting either testimony, we would decide that *'the* document to which *they* refer' has *no* reference, or, putting it in the material mode, that the document (before the court) has a duplicate—that they are referring to, or talking about, different pieces of paper. (I do not want to suggest that this is the only way we detect failure of reference, only that it is an important one.)

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[1] Though it is difficult to imagine what the experiences of *inhabitants* must be like if *they* are duplicated in analogous ways. Probably the same faults could be found with this model as are found with Goddard's.

It seems then that Mr. Goddard's ingenious account of a 'different' universe either reduces to an account of our own where temporal references fail in a systematic way, or to one in which uniqueness of reference is impossible.

There is always a danger in describing 'other universes' of *under* describing them. It is not always that we would, had the world been different, have *had* to say 'p'; often we would be unable to say anything *at all*. And without *any* saying, there could be no logic.

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## II. By L. GODDARD[1]

I want to agree with almost everything that McGechie says and yet disagree with the interpretation he puts on the various points. Indeed I want to say that he is arguing my case for me, though the overtones suggest that he is not.

First, however, let me acknowledge the two mistakes. It is clear that I cannot have everything happening twice and at the same time have the stone undergoing unique changes of position. In fact the existence of unique discontinuity-points is the condition for everything else to happen twice. But I think that this commits me only to rejecting the rash generalization, not to denying the possibility of the universe I want to describe. At all events, McGechie does not use the point to rule the discontinuous universe out of order, and I shall not. What matters to me is the behaviour of the stone, and I am happy enough to modify the rest to make it fit. Similarly on the matter of the two clocks in our universe which simulate the measurement of time in the discontinuous one; this, certainly, is inadequate. An infinite set of clocks, each lagging a half-hour behind its predecessor, would seem to be required. Here again, however, it is the behaviour of the clocks in the discontinuous universe that matters, and their movements, like the stone's, are *ex hypothesi* discontinuous. These points, therefore, though important, do not seem to me to be crucial.

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[1] Some of the things I say here have come out of discussions I have had from time to time with D. R. Grey.

I take the crucial points to be, first, that there are no empirically distinguishable differences between the discontinuous universe and ours; and secondly, that even if we grant a "real" difference (which cannot be observed empirically from within either universe), it is not the law of non-contradiction that fails but uniqueness of reference.

The first point is one that I am anxious to make myself. The man who "sleeps" through the discontinuities would have experiences indistinguishable from ours; so too, as McGechie shows, would the men who live in the discontinuous universe and who are subject to the same sort of discontinuities as the stone. The clocks go back and they go back; their blood particles are where they were half an hour before and their thoughts and memories are those of half an hour ago. But if this is so, these men *could* be us. That is to say, we could suppose our universe to be like this, and with propriety deny the law of non-contradiction (if indeed it is the law of non-contradiction that fails). There are thus no empirical tests which will enable us to discover what in fact *our* universe is really like. The difference between our universe and the discontinuous one is not the kind of difference that a physicist could spot by careful observation. For the spatio-temporal assumptions which we commonly take for granted provide the conditions within which the physicist operates. Without them there could be no physics, and within them they cannot be challenged physically. Then what *really* is the difference? Simply that we could, if we chose, talk intelligibly within our universe in a fundamentally different way. And this means that our ordinary way of talking is a theory about the world, not an unadulterated description of it. Built into language is a metaphysic which makes physics and the rest possible. The metaphysic constitutes the framework, and part of the framework is the law of non-contradiction.

We can have a different metaphysic but we cannot have none. If we did not have presuppositions about the nature of an object, we could not have part-logical, part-metaphysical principles such as 'Two objects *cannot* occupy the same place at the same time' and 'An object *cannot* be in two places at once', and we could not have physics. We could have a different metaphysic, different principles and a different physics; but not the latter without the former. And though it might be said that we "see" the truth of such principles in every observation that we make, and so they are not really metaphysical, we only do so because we know how to see; *i.e.,* because we know what counts as an object.

But the justification of what counts as an object is not a physical one.

Consider, for example, the dilemma posed by the two men one of whom says "When you cross your eyes there appear to be two objects but really there is one" and the other of whom says "When you uncross your eyes there appears to be one object but really there are two". Looking with uncrossed eyes, and *knowing* that an object cannot be in two places at once, are canons of the perceptual methodology which creates normality. Metaphysical principles (and logical ones stemming from them) define perceptual contexts but are not part of them. There is a paradigmatic looking. When we teach a child the meaning of 'water' we teach him what water is. We teach him how to see correctly as well as how to speak correctly. We make him conform by teaching him in a certain way; and so we make him normal by teaching him what really is.

But what *really is* could be different. For if we change our metaphysical presuppositions which are built into our ways of perceiving, *a fortiori* we change our ways of perceiving, hence our ways of thinking and talking, hence our physics, geometry and logic. So we would in fact *see* differently if we denied the principle of non-contradiction, but we should never know this since it would be seeing as it should be within the methodological framework implied by the denial. The framework would move, but we do not see frameworks; we take them for granted; we operate in terms of them; we use them as norms and refer to them in terms of 'musts' and 'cannots' in matters of dispute.

So I want to say that our spatio-temporal conventions are indeed conventions; they could be different. And I want the discontinuous universe to illustrate a logically possible shift in our metaphysical views about (or our methodological techniques with respect to) the nature of space, or time, or objects. But because metaphysical presuppositions condition our ways of discovering and talking about what happens, no empirical investigation of what happens could detect changes in them. This is what we mean when we say that metaphysical principles (and logical ones) are independent of what happens in the world, or that they are unverifiable. Since they are independent of the world, however, it is always possible to deny them; and since they are not independent of our way of talking about the world, if we are to talk intelligibly, we cannot manage without them. (That is why philosophical appeals to ordinary language are pointless, for they are simply appeals to the metaphysics of the *status quo*; and the elimination of metaphysics by nonsense criteria is pointless, for it is an elimination of everything; if we eliminate metaphysics, nothing can be said.)

Hence I can agree with McGechie when he says, "Really the Goddard universe *is* a different one from ours, but any investigation of ours will fail to find respects in which it differs from Goddard's and vice versa", and claim that it makes my point for me. *Really*, it is a different universe; and *because* we could never *discover* this, we could live within it. And in answer to the further point that my account of a different universe "reduces to one of our own": it does not reduce, it just is a different account; but certainly an account of our own.

But this is to ignore the second difficulty which McGechie raises. For it is not just that it reduces to an account of our own, but that "it reduces to an account of our own where temporal references fail in a systematic way, or to one in which uniqueness of reference is impossible". Thus, though the stone behaves in our experiential "gaps" in the way I have suggested, it is not the principle of non-contradiction that fails but uniqueness of reference. And this is because "we 'use' the law, as it were, to *detect* ambiguity of reference".

It seems to me, however, that we only can use the law in this way in order to educate the borderline abnormal perceivers into a proper and respectful obedience, and that the man who wants to say "Movement is really discontinuous" is in a different category.

Thus, given the presuppositions which we have about objects, then the principle that no object can be in two places at once is unassailable and can be used as a big stick. It is one of the norms to which we refer in order to settle matters of dispute, and certainly we would say that "*the* document to which *they* refer has *no* reference, or . . . that the document has a duplicate . . . that they are talking about different pieces of paper". But if we shifted our metaphysical ground in such a way that *everyone* was guilty of ambiguity, then it might well be that we should be more inclined to reject the principle in order to save the possibility of discourse. Big sticks are only useful in persuading the odd man out to conform; they cease to be useful if the *status quo* collapses.

Suppose, for example, that an observer from our universe "looks down" on the discontinuous universe and sees the stone rolling down the hill—apparently every bit like one of our stones. Momentarily it then "appears" to be in two places at once. It then continues to roll down the hill, but from the higher of the two points. The same sort of thing happens again and again. The observer clearly cannot ignore this peculiar behaviour and it is natural for him to set up explanatory hypotheses.

He begins by saying that *the* stone is in two places at once. But when he reports this to his superiors, he cannot make himself understood. 'The stone', or 'The place where the stone is' is ambiguous. "It only *seemed* as if there was one stone," they say; "But there *must* have been two *really*—a trick of the light, perhaps?" Here, the principle of non-contradiction is being used to detect ambiguity of reference; and because it is tied to a particular way of looking, because it is part of the methodology of perception, it is a criterion of what really is the case no matter what one thought to have seen to be the case. So far, therefore, the situation is similar to the one McGechie cites.

Yet the observer's report is not on all fours with that given by the disputing litigants in McGechie's example. They are much more in the position of the man who sees a pink elephant in his bathroom. That there are really two pieces of paper; that really the pink elephant was an hallucination; these are explanations which fit logically into the existing structure. Indeed they are part of it, since they are techniques created to explain away the borderline cases. But the observer who reports back his odd experiences is challenging the whole structure of the existing perceptual methodology, since the reply "There are really two stones", which invokes the principle of non-contradiction as a criterion of normality, implies the rejection of other similar criteria.

For suppose we say that really there are two stones, one a half-hour's journey behind the other, such that each one is visible for an hour, stops, and disappears for an hour. As it stops, the second appears, rolls for an hour, stops and disappears for an hour; and so on. This *could* happen within our universe; on the face of it it fits. But, of course, it leaves too much unexplained. For the kind of disappearance is very different from any that has been experienced before. None of the tests work. The observer finds that it is pointless to look under the bush or in the concealed hollows. To save the meaning of 'disappear' he therefore supposes that objects in this universe become invisible and intangible at regular intervals; and now he has lost the meaning of 'object'. For it is not the kind of invisibility and intangibility which occurs when he turns his back on the table and walks three paces forward. He knows how to 'regain' the table, but nothing he could do would be relevant to the determination of the missing stone's position. He cannot say where the stone is nor yet what is happening to it. It is inaccessible; but then, not in the way in which the bird's nest on the ledge is inaccessible. Much worse than any of this, however, is the fact that when the second stone reaches the bottom of the hill, it merges with the first. And now he has to give up a methodological principle every bit as fundamental as the law of non-contradiction. For he now has to say that two objects can occupy the same bit of space at the same time. Thus he loses the meaning of 'object' again.

It seems, therefore, that if we are to save the principle of non-contradiction, or equivalently, if we regard it as unassailable and use it as a criterion to detect ambiguity, we pay a price at least equal to that which we would have to pay if instead we rejected it. It may be, of course, that there are alternative explanations which would not involve us in any logical reorientation, but this seems unlikely. We might perhaps say that there is one stone and its shadow, since shadows do merge, in one sense of 'merge', with objects; and the peculiar behaviour of this particular shadow might be explicable in terms of a different and complicated theory of light—which means a different physics, not a different logic. But shadows do not hurt our toes when we kick them, nor do they 'bump' when they reach the bottom of the hill. And if we say with McGechie that there are *two* ghostly shadows, we have the additional difficulty that they are shadows *of* an 'object' which is not only unobserved but unobservable.

Whatever line we take, therefore, we seem to be faced with a logical (methodological? metaphysical?) shift of one kind or another. Reference, as we know it, fails in each case. We cannot simply choose to say: reference fails; and leave it at that. However we deal with the situation, some words will be ambiguous in terms of the old criteria and will have to find a new meaning in terms of different criteria. What these are will depend on the logical shift we are prepared to make; but we must make *some* logical shift; hence some verbal one.

So if someone says, "Really, movement is discontinuous", we cannot deal with him as a borderline pink-elephant case. For he is not pecking away at the boundaries but digging a hole under the foundations, and we have no way of making him conform. We can only say, "Really, it is not". That is, we can use the whole metaphysic as a criterion of conformity, but not just a part of it. He is on a par with the man who not only sees the pink elephant but who also strokes it, smells it, feeds it and pets it—all within a minute bathroom. He is so abnormal that he cannot be rehabilitated. He is as right as we are; and as wrong. We stand to him in the same sort of relationship as Locke to Berkeley on the question of substance; yet neither of us is speaking nonsense.

McGechie's universe in which *everything* happens twice seems to me to raise the same sort of difficulties; though here, I think, there is more empirical persuasion to talk of two things (ghostly duplicates) rather than one. In McGechie's cinema, when movement is taking place on the screen, the two films superimposed on each other show two things always, whereas in mine this only happens at the discontinuous moments. At all other times there is one thing. The picture on my cinema screen is normal except that once each hour it overlaps momentarily with the picture as it was a half-hour earlier, and then continues from that point. For this reason I think that it would be more natural to speak in my cinema of one object behaving occasionally in such a way as to violate the principle of non-contradiction; in McGechie's, on the other hand, it would seem to be more natural to speak of two objects which, when they come to rest, violate the principle that no two objects can occupy the same bit of space at the same time. However this may be, each of us has to face up to some major logical breakdown, not just a verbal one; and that is the only point I want to make.

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# CRITICAL NOTICE

INTENTION. By G. E. M. Anscombe. Oxford, Blackwell, 1957. ix, 93 p. 10s. 6d. (U.K.).

This book attempts to show in detail that the natural and widely accepted picture of what we mean by an intention gives rise to insoluble problems and must be abandoned. Employing the method and style of the later Wittgenstein, Miss Anscombe at first assumes, and draws out the consequences of, this view in order to lead it *ad absurdum*. After piling up an impressive number of difficulties, she offers her own account. All this is done with a wealth of detailed argument, many ingenious, helpful, illuminating, and often very amusing illustrations, affording the reader immediate and gradually deepening understanding. Nevertheless, the book as a whole is baffling and the outline of its over-all theme not easily discerned. I should probably not have persevered in looking for it if I had not undertaken to write this review, yet perseverance was well worth while. Perhaps it is not too self-centred to assume that some readers of this journal have had similar difficulties and will be helped if I state simply what I take to be Miss Anscombe's main aims and contentions.

The chief target of Miss Anscombe's attack is the view "that events which are characterisable as intentional or unintentional are a certain natural class, 'intentional' being an extra property which a philosopher must try to describe" (p. 84, cf. also p. 29). Certain seemingly indubitable "facts" (such as that "the question what a man's intentions are is authoritatively settled only by him") then "conspire to make us think that if we want to know a man's intentions it is into the contents of his mind, and only into these, that we must enquire; and hence, that if we wish to understand what intention is, we must be investigating something whose existence is purely in the sphere of the mind" (p. 9).

Of the many difficulties entailed by this view, the following two are the most damaging. It can, in the first place, give no plausible account of what appears to be a plain fact, namely, that we often know, without having to rely on empirical observation, what we are at a given moment engaged in doing intentionally. When I tell someone who cannot see what I am doing, that, say, I am opening the window, my remark will often constitute knowledge, but not knowledge merely by observation. "I don't say the words like this 'Let me see, what are my movements bringing about? Ah yes! The opening of the window' " (p. 51).

Contrast the above case with the following where I do have knowledge only by observation: "I open the window and it focuses a spot of light on the wall. Someone who cannot see me but can see the wall, says 'What are you doing making that light come on the wall?' and I say 'Ah yes, it's opening the window that does it' or 'That always happens when one opens the window at midday if the sun is shining' " (p. 51). Thus, there are two quite different ways of coming to know: by empirical observation (as in the case of the spot of light), and in the manner in which we come to know our intentional actions (as in the case of our opening the window). But, in Miss Anscombe's opinion, this gives rise, in the case of intentional actions, to a serious difficulty: Either we say that there is only one object of knowledge or that there are two. We cannot say there are two, for "what can opening the window be except making such-and-such movements with such-and-such a result? And in that case what can *knowing* one is opening the window be except knowing that that is taking place?" (p. 51). But we cannot say that there is only one object of knowledge, for how could there be "two different knowledges of *exactly* the same thing?" (p. 51). In Miss Anscombe's opinion, "it is the difficulty of this question that has led some people to say that what one knows as intentional action is only the intention, or possibly also the bodily movement; and that the rest is known by observation to be the *result*, which was also willed in the intention" (pp. 51-2). I doubt myself whether this is a real difficulty, let alone whether it has often been felt as one. We can know by sight *and* by touch that someone has fat knees, by the way we feel *and* by looking at the thermometer that the room is hot or that we have a temperature, etc. I think the difficulty that drives people into the view that what is known without observation is only the intention, whereas the rest is known by observation, is due rather to the fact that intentions do not always get executed.

Whatever may be the origin of this traditional answer, it gives rise to the second major difficulty involved in the received account, the difficulty of making sense of the notion of 'executed intention'. Yet this is an essential logical dimension of the concept of intention. On the traditional view, the connection between the intention of opening the window and the window getting opened is indistinguishable from the connection between the mere wish that the window would open and it getting opened, but there is all the difference between an intention being executed and a wish coming true. It will not do to say that in the case of the executed intention there is a causal connection between the bodily movement of the person and the event, for the problem lies in the connection between the "internal" intention and the "external" bodily movement. Either some external "vehicle" (p. 52) of the intention is guaranteed by the intention or the intention must remain "a bombination in a vacuum" (p. 52; cf. also pp. 66, 86). If we cannot will or intend the opening of the window, then we cannot in that sense will or intend the movement of a muscle either (p. 52).

This point is of great philosophical importance and is supported by Miss Anscombe with considerable skill (cf. in particular the brilliant example of the man who is learning to drop his arm at the acceleration of free fall, on pp. 53-4). It gives weight to her attack on the kind of philosophical position (e.g. Phenomenalism, cf. p. 67) which attempts to describe everything in terms available to the prisoners in Plato's cave, that is, in terms of observers capable only of passively watching, correlating, and perhaps predicting on the basis of regularities, the items of the passing show. The traditional theory reduces the will to yet another observed occurrence in the flux, thus making a complete mystery of action. Miss Anscombe's examples are designed to rid us of the temptation to "reduce" our ability to move certain parts of the body at will to the mere observed discovery of a regular connection between certain mental occurrences (intentions) and certain physical ones (bodily movements). (Cf. pp. 51-2.)

To this traditional view Miss Anscombe opposes her own, whose chief features are: (i) that all the uses of words of the word family 'intention' are based on, and are to be understood by way of, 'intentional action' (pp. 7-9, pp. 30-33); (ii) that the expression 'intentional action' refers to a form of description of events which, when correctly described in this form, *are* executed intentions (pp. 84, 86); (iii) that though our knowledge of what we are doing intentionally is knowledge of what happens in the world, it is nevertheless not, as it would appear necessarily to be, knowledge by observation; for it is not speculative knowledge at all, but practical knowledge (pp. 53, 57, 86-7).

Miss Anscombe introduces her subjects under three linguistic headings, expression of intention for the future, intentional action, and intention with which something is done (p. 1). She immediately rejects the view, perhaps taken by some linguistic analysts, that, in these different uses, the word 'intention' is used in different senses (p. 1). She then examines, as the most useful approach to the discovery of how we ever know that a given person intends to do a certain thing, the cases in which we can simply look at a man and say at once what he is engaged in doing, as when "he is sitting in a chair and writing" (p. 8), for in most of these cases "you will be reporting not merely what he is doing, but *an* intention of his—namely, to do that thing" (pp. 8-9). Her main point is that what a man actually does is not the very last but the very first thing we need to consider in our enquiry into a man's intentions (p. 9).

She then takes up the question of what distinguishes intentional from unintentional actions. Her main point is that the expression 'intentional action' has reference to a form of description of events (p. 84). It is a "vital", "animal" (p. 85) description, "going beyond physics" (p. 85). Descriptions of events such as 'the water reached boiling point' or 'the thaw caused a number of avalanches' are purely physical descriptions. On the other hand "to speak of the wind as picking things up and putting them down again is to animalize it in our language, and so also if we speak of a cleft in a rock as holding something; though not if we speak of something as held there by the cleft" (p. 85).

The chief logical features of this vital or animal form of description are knowledge and appetitiveness (p. 85). A man's action could not be said to be intentional if he can truthfully say that he did not *know* he was doing that. Of course, what he does may be equally truthfully describable by a great many different descriptions, hence intentional under one, not intentional under another. He may for instance know that he is sawing a plank (and intend to do that) but not know that he is sawing Smith's plank or that he is sawing oak (p. 11) and therefore not intend to do that.

It might now be thought, since some human actions are intentional and others unintentional, that events which could be either formed a natural class and that 'intentional' was the name of "an extra property which a philosopher must try to describe" (p. 84). If so, it is natural to look for that property in the basic physical occurrence (say, the movement of muscles or molecules (p. 29)) at the time the intentional action is performed and to note its absence from unintentional actions. Against this, Miss Anscombe rightly insists that the overwhelming majority of descriptions by which we refer to unintentional actions have their existence in our language only because they are often used to refer to intentional ones. We have a phrase for an enormously complicated event such as 'paying a gas bill' or 'preparing a massacre' (p. 82) only because these human bodily movements and their results, etc., are of interest to us and can be engaged in and brought about intentionally.

"The tree waves in the wind; the movements of its leaves are just as minute as the movement of my hand when I write on a blackboard, but we have no description of a picked-out set of movements or a picked-out appearance of the tree remotely resembling 'She wrote "I am a fool" on the blackboard' " (p. 82). If, however, legible marks appeared on a wall as at Belshazzar's feast, then we could describe this occurrence as 'writing on the wall', but we can do this only because intentional writing frequently occurs (p. 83). Unintentional action is no more correctly described as a kind of action than tin soldiers as a kind of soldiers.

Hence, it is not the case that being intentional is a property that is added to and being unintentional a property that is absent from the natural class of actions. It is the case rather that if an event is described by one of the innumerable descriptions (e.g. writing, sawing, offending) which are logically dependent on the existence of the form of description 'intentional action', then there is a presumption that that event is an intentional action, an executed intention. One does not then have to prove that the action is intentional; that goes without saying. A proof is required only if the action is *not* intentional (p. 86).

This point becomes clearer when we raise afresh the question which proved so troublesome to the traditional view, namely, how it is that we know without observation not merely our intention but also what is actually going on in the world, how we know not merely that we intend to do something but also that our intention is being executed. Miss Anscombe's answer to this problem is that we know it because we have practical knowledge of it. We must abandon the "incorrigibly contemplative conception of knowledge" (p. 57) which has dominated modern philosophy. We shall then not be puzzled how it is that there can be "two knowledges" (pp. 51, 57) and yet only one object of knowledge. We shall not then "look hopelessly for the different *mode of contemplative knowledge* in acting, as if there were a very queer and special sort of seeing eye in the middle of the acting" (p. 57). We shall understand that "in any operation [action] we really can speak of two knowledges—the account that one could give of what one was doing, without adverting to observation [practical knowledge]; and the account of exactly what is happening at a given moment (say) to the material one is working on" [speculative knowledge] (p. 88).

To see this more clearly, we must try to make explicit Miss Anscombe's implied conception of the nature of knowledge. There is a wide sense of 'knowledge', not dependent on the existence of language, in which animals can be said to have knowledge, and a narrow one in which it involves the correct use of words. During the learning process, when pre-linguistic knowledge is acquired, speculative and practical knowledge cannot be separated, though they can be distinguished in thought. Knowledge can only be acquired by a creature that has both the capacity of sensory discrimination and "volition", that is, appetitiveness, or the capacity for wants (pp. 67, 85). An animal cannot be said to be trying to get something unless at the same time it can be said to know the thing, that is to perceive it, to identify or recognise it as being of the sort wanted (p. 67). Conversely, a creature cannot be credited with, say, colour-discrimination unless it can learn to do things (fetching, carrying, placing them) with objects picked out by their colour (p. 67). A creature has practical knowledge if it has such "skills" as fetching, carrying, placing, etc. It has speculative knowledge, if it can discriminate, say, red objects, blue objects, and so on, in the context of such tasks.

Language introduces the new factor of communication. Having acquired speculative and practical knowledge in various fields, the child must now learn the linguistic apparatus for making joint use of it. On this level, knowledge consists in the ability to use the linguistic apparatus in the completion of speculative or practical tasks without making mistakes. Practical tasks are those in which a person is concerned to make the environment correspond to words (whether orders or instructions given by someone else or formulations of his own intentions); in speculative tasks, he is concerned to make the words correspond to the environment (pp. 56-7, 86-7). It is the difference between the sorts of task and the corresponding sorts of mistake involved that distinguish speculative from practical knowledge. A person has practical knowledge if he is able, without making a mistake, to complete practical tasks. He has speculative knowledge if he is able, without making mistakes, to complete speculative tasks. Miss Anscombe quotes from Aquinas, "Practical knowledge is 'the cause of what it understands', unlike 'speculative' knowledge, which 'is derived from the objects known' " (p. 87). An expressed intention is related to its execution in very much the same way as an order to its execution. Both can be mis-executed (p. 57). We make a mistake wherever there is a discrepancy between the words and the events, but it is a speculative mistake, if the fault lies with the words; a practical mistake if it lies with the performance (pp. 5, 57, 82, etc.).

This may now be used to solve the problem of how we can know without observation that we are intentionally doing something, say opening the window, without having to postulate "a very queer and special sort of seeing eye in the middle of the acting" (p. 57). The answer is, of course, that our knowledge of our intentional actions is practical. Practical knowledge is thus necessarily involved, because necessarily exercised, in intentionally doing something. Furthermore, what is going on, say the getting opened of the window, is the execution of one's intention only in as far as one knows *under that description* what one is doing (p. 86). But when someone says, "I wrote 'I am a fool' on the blackboard with my eyes shut", ought he not to have said: "this is what I am writing, if my intention is getting executed"; instead of simply: "this is what I am writing"? (p. 81). The answer is No, because, "surprising as it may seem, the failure to execute intention is necessarily the rare exception" (p. 86). It must be the rare exception or else the agent could not be said to have practical knowledge, "to know his way about the matters described in the description under which his action can be called intentional" (p. 88).

There are two other related features of the form of description 'intentional action' which are explored at length by Miss Anscombe: the fact that what a person or animal does intentionally can be described by a series of "enlarged descriptions" (p. 86) and that these display an order and goal-directedness corresponding to the appetitiveness we have already noted. "We describe what *further* they are doing *in* doing something (the latter description being *more* immediate, nearer to the merely physical): the cat is stalking a bird *in* crouching and slinking along with its eyes fixed on the bird and its whiskers twitching" (p. 85). Of course, the enlarged description is not peculiar to intentional action, "for enlarged descriptions are possible of any event that has describable effects, but to this is added the cat's perception of the bird and what it does if it catches it" (p. 86), the latter being our reason for saying that it *wanted* the bird. (Cf. the central example on p. 37.)

All this now explains why it is that Miss Anscombe can define intentional actions as those to which a certain sense of the question "Why?" has application (pp. 9, 11, 28) for, while certain answers, e.g. "I was not aware I was doing that" (p. 11), refuse that question application and therefore (if true) reveal the action as unintentional, those which give it application display the above order and goal-directedness by way of a series of enlarged descriptions (p. 87). "Jones is moving his arm up and down"—"Why?"—"To operate the pump"—"Why?"—"To replenish the water supply"—"Why?"—"To poison the inhabitants"; and so on. (pp. 37-40.)

This section (pp. 9-28) is the least satisfactory. It is very condensed, shows signs of either having been composed in haste or embodying material written at different times and expressing incompatible views. Miss Anscombe, for example, does not seem able to make up her mind whether to say that intentional actions are or that they are not subject to mental causation. By 'a mental cause' she means one which "itself *qua* cause (or perhaps one should rather say: the causation itself) is in the class of things known without observation" (p. 16). Thus, in several places (pp. 19, 24) she implies that intentional actions are subject to mental causation. "Intentional actions are not marked off just by being subject to mental causality, since there are involuntary actions from which mental causality is not excluded" (p. 24). In other words, she appears to say that the class of actions subject to mental causality includes both the class of intentional actions and a certain subclass of the class of involuntary actions. Yet in the very next sentence she appears to contradict this. "Intentional actions, then, are the ones to which the question 'Why?' is given application, in a special sense which is so far explained as follows: the question has not that sense if the answer is evidence or states a cause, including a mental cause". That is to say, if the answer to the question "Why?" states a mental cause of the action (e.g. "the leap and loud bark of that crocodile made me jump" (p. 15) or "The martial music excites me, that is why I walk up and down" (p. 16), etc.), then the question "Why?" has not the sense in which alone the application of this question shows the action to be intentional. It takes some time to see that this is not a contradiction, for it is just barely conceivable that a given action should be such as to allow true answers to the question "Why?" one of which refuses and one of which gives application to it in the appropriate sense. It is not clear to me, however, whether Miss Anscome thinks there are such cases. She does not appear to mention one. Besides, walking up and down, for instance, may well be intentional even if one does it because the martial music excites one and there is no other true answer which gives application to the question "Why?" in the appropriate sense.

Miss Anscombe also raises the question of how we distinguish between "a reason" and "a cause" (p. 23) and claims that in very many cases the distinction would have no point, that reason and cause are not everywhere sharply distinct notions (p. 24). However, her examples and arguments certainly have no tendency to show this, for she fails to keep apart different senses of the question, "Is this a reason or a cause?". One may, for instance, use these words to ask whether the agent acted after deliberation or on impulse. She has that sense in mind when, in considering the case of the person who turned round because someone said "Boo!", she comments: "Forced to choose between taking the noise as a reason or a cause, one would probably decide by how sudden the reaction was" (p. 23). Alternatively, one may be asking whether the agent thought of the necessary and sufficient condition of his action as being a means to his end. She appears to have that sense in mind when she comments, "the more the action is described as a mere response, the more inclined one would be to the word 'cause'; while the more it is described as a response to something as *having a significance* that is dwelt on by the agent in his account, . . . the more one would be inclined to use the word 'reason' " (pp. 23-4). Lastly, one may be asking whether what prompted the agent to act was really a reason or merely thought to be so by the agent. If a daughter is 25 years of age, we shall probably think that her mother's order not to wear lipstick was not a reason for her not to have used any, though of course she may have thought so and it may therefore have been *her reason* for not using any.

Miss Anscombe never considers that last sense at all. Yet, with these three different senses in mind, the distinction between cause and reason will remain clear, even where she thinks it vanishes, as in her example where the guest hangs up his hat on a certain peg, because the host said, "Hang up your hat on that peg" (p. 23). We are unable to say "whether cause or reason" only if we cannot keep apart the conflicting considerations belonging to the different senses of the question. Of course, if she merely means to say that we can speak of the same occurrence or fact as both the cause of the action and the person's reason for the action, then I agree. But this is different from saying that the *notions* are not sharply distinct.

Miss Anscombe also attempts to explain the difference between intentions and motives. She divides motives into three kinds, backward-looking, forward-looking, and interpretative. Explanations by backward-looking motives, such as revenge and gratitude, are those in which we explain an action (or abstention) by giving as a ground a concrete past event that is good or bad for the person aimed at (it may be oneself, as with remorse) (p. 20). In explaining revenge, we would simply mention that it "was harming someone because he had done one some harm; we should not need to add to this a description of the feelings prompting the action or of the thought that had gone with it" (p. 20). In this last respect, backward-looking motives differ from interpretative motives such as love and friendship, for in the explanation of the latter feelings and thoughts must be mentioned (p. 20). Lastly, forward-looking motives are those, such as fear, gain, despair, which are intentions. "For example, to say that someone did something for fear of . . . often comes to the same as saying he did so lest . . . or in order that . . . should not happen" (p. 21).

This seems to me a confusion, for her distinction is not, as

Miss Anscombe thinks, a distinction between different types of explanation on the basis of different kinds of motive, but a distinction between different types of explanation, each of which can be given in connection with any and every kind of motive mentioned by her.

Consider, again, revenge. There are three elements in the sort of situation in which we can talk of revenge: (i) A past event which has upset the balance of good and harm, i.e. someone has inflicted harm on the agent or a person close to him. (ii) This gives rise in the agent to a desire either to do a particular thing to the harm-doer (which as a matter of fact is thought to harm him and thereby to restore the balance) or, if the concept of revenge is already institutionalised, to a desire to do *whatever* is considered necessary to restore the balance (say, fight a duel). (iii) The state of affairs resulting from the agent's action, which, if successful, is called 'revenge'. Now, this act of revenge can be explained by three quite different types of explanation, corresponding to these three different elements.

(A) We can give an explanation in terms of a "concrete past event" (cause): "because he killed the agent's brother". This constitutes an explanation only in conjunction with the other unmentioned but implied elements: that this past event is of a sort that sets up a desire such as that mentioned under (ii) and that the action to be explained is thought by the agent to satisfy that desire.

(B) We can give an explanation in terms of the agent's *motive*: "he did it out of revenge", "he did it out of friendship". It makes no difference whether the motive is forward-looking, backward-looking, or interpretative. In each case, this sort of explanation (the paradigm of motive-explanations) mentions the same sort of thing, namely, the agent's desire to bring about something, a desire which was caused by a particular preceding event not mentioned but merely implied. It follows from this that expressions such as 'out of revenge', 'out of gain', though not 'out of gratitude' and 'out of friendship', are used elliptically. Miss Anscombe denies this (p. 18) because she overlooks the logical differences between expressions such as 'revenge' and 'gratitude'. Yet, they are quite straightforward. As Miss Anscombe herself notes (p. 20), killing a man *is* revenge, but it *does not show it*, whereas doing good to a man *shows*, but *is not* gratitude. Gratitude is *the desire* to do good to someone who has done good to one, not the action satisfying that desire. Revenge, on the other hand, is *not the desire* to do harm to someone who has harmed one, but *is the action* satisfying that desire. 'He did it out of revenge' is clearly elliptical for 'he did it out of a desire or thirst for revenge', since these two mean the same, whereas 'he did it out of gratitude' is not elliptical for 'he did it out of a desire for gratitude', since these two mean something quite different. Miss Anscombe wrongly equates revenge, remorse, pity, and gratitude, instead of desire for revenge, remorse, pity, and gratitude. The analogues of revenge are, more nearly, reparation for damage done, affording comfort for suffering undergone, and returning benefits received. The former are desires for achievements of a certain sort, the latter are the achievements themselves. And what is true for the backward-looking motives of revenge and gratitude is true also for forward-looking ones such as gain and fear, and interpretative ones such as friendship and admiration. To say that someone did something out of any one of these is to explain his behaviour by reference to a range of desires grouped under that name which have been aroused by a certain event in the past and whose satisfaction is thought to be brought about by the action to be explained.

(C) Lastly, we may explain a person's behaviour by a reference to what his action is designed to achieve. Sometimes, the achievement has a name, e.g. revenge, but usually it has not. In the latter case, we have to be specific and say "lest so-and-so should happen" or "in order that so-and-so should not happen". It seems to me therefore that there is at least as much to be said for as against the usual distinction rejected by Miss Anscombe (p. 18) between a man's intention as *what* he aims at or chooses (e.g. harm or good to someone) and his motive as what determines the aim or choice (e.g. thirst for revenge or gratitude).

It is a pity that these discussions occur early in the book where they tend to confuse and dishearten the reader. I hope I have said enough about the remainder of the book to persuade at least some who would otherwise have desisted to persevere in reading this highly stimulating, original and penetrating book.

For reasons of space I have said nothing about another long section on Aristotle's practical syllogism (pp. 57-81) which is introduced both for its own sake and because it "describes an order which is there whenever actions are done with intentions: the same order as I arrived at in discussing what 'the intentional action' was, when the man was pumping water" (p. 79). It contains many true and novel observations about what it is to want something and about pleasure, as well as some that are highly questionable.

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Aesthetics: Problems in the philosophy of criticism. By Monroe C. Beardsley. New York, Harcourt, Brace, 1958. xvi, 614 p. $9.50.

Problems in Aesthetics; an Introductory Book of Readings. By Morris Weitz. New York, Macmillan Co., 1959. ix, 697 p. $6.90.

Aesthetics, Professor Beardsley reminds us, "has long been contemptuously regarded as a step-sister within the philosophic family". Yet many philosophers nowadays would probably agree with Bernard Mayo that philosophy is best explained in terms of a "three-tier model", in which the first tier is occupied by people who simply act, the second by people who "criticise, evaluate or comment on the actors", and the third by philosophers, who "are neither doing, nor thinking about what is done, [but] thinking about the criteria or principles of the critics, about the concepts used in their thinking".[1] It would seem to follow that aesthetics is the most obvious example of a philosophical activity, since it is the one that fits the model with least strain. Moreover, there can be few departments of life in which those on the second tier seem so clearly in need of the services of those above them. Beardsley begins with some typical quotations from reviewers of books, records and art exhibitions: they are, as he points out, "fairly rich in unclear terms and challengeable assumptions".

There is need, then, for a book of this kind, which, taking as its data the things critics actually say, sets out to clarify their terms and drag their assumptions into the open. Beardsley does this job well. Current critical usage, he suggests, is so chaotic that "if we are going to require that at least our own way of using the term shall be clear and unequivocal, we cannot hope to frame a definition that will correspond to all common usages. Therefore, we must . . . single out the usage, even though it is not exact, that seems most worth preserving" (p. 166). In this way he is able to give a coherent account of such concepts as "form", "structure" and "texture", and to apply them in turn to painting, literature and music, with many examples of their application to each of these arts. He also makes some interesting proposals for distinguishing between such terms as "portrayal" and "depiction" in art, or "theme" and "thesis" in literature. As for unacknowledged assumptions, Beardsley finds these (in the form of

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[1] *Ethics and the Moral Life*, pp. 10-11.

"theories") lurking behind every thicket in the critical undergrowth. Patiently he hunts them into the open; and we get expositions, accordingly, of the Emotive Theory, the Supervenient Theory, the Literalist Theory, and the Controversion Theory of metaphor; the Diversion Theory and the Fusion Theory of the relation between design and subject; the Proposition Theory, the Revelation Theory (with the Universalist and Essentialist Versions of it) and the Intuitionist Theory of truth in art; and so on.

All this is, in a sense, preliminary work. In spite of its length, this book does not do much more than sort out the problems and state them clearly. It takes about four hundred pages to reach the topics which most writers on aesthetics treat as central, if not indeed as the whole of the subject: subjectivism and objectivism, the nature of aesthetic experience, and the like. Beardsley's treatment of these, though always clear and sensible, may strike some readers as rather superficial. In aesthetics, as in ethics, it is hardly possible to dismiss subjectivist or emotivist theories in a chapter or two. One may wonder, too, whether more argument is not necessary to establish Beardsley's contention that the canons of criticism (unity, complexity and intensity) are ultimately the same for all the arts, and that these headings are not "so vague or so general that they could cover every logical possibility" (p. 466).

It would, however, be a mistake to put too much emphasis on these criticisms. Beardsley's method throughout is to concentrate on stating each problem clearly, and then to outline the possible solutions, and finally to give us a brief but often convincing statement of his own position and his reasons for adopting it. Naturally this procedure is most illuminating when the problem is least familiar. Consequently this is essentially an introductory book. But to say this is not to disparage it; for what aesthetics most needs is just this careful sifting and clarification. Nor is it easy to do. Philosophers who have hitherto found aesthetics a peculiarly irritating and disheartening subject will be grateful to Beardsley. I know no introduction more likely to stimulate the reader to do more work on the subject.

The reader is also given an unusual amount of very valuable advice about this further work. Each chapter has a lengthy section called "Notes and Queries", in which books and articles (and not only the obvious ones) are cited and briefly discussed. Between them, these notes constitute a bibliographical guide to aesthetics which it would be difficult to equal, either for its fullness or for the helpfulness of its comments.

I would have said that this book was obviously intended as a text-book for students if I had not read Beardsley's strictures on "the intentional fallacy". As he points out, one knows nothing of an author's or an artist's or a musician's intentions except what one gathers from the work of art itself. Any statement about intention, then, can (and, in the interests of clarity, should) be put in the form of objective statements about the work of art itself; and I have tried to do this in the two preceding paragraphs. One can be less cautious about Professor Weitz's anthology; for he tells us in the preface that it "has been assembled in the conviction that beginning students in aesthetics should have available, in the same volume, some of the excellent aesthetic analyses by philosophers and others written before the twentieth century as well as those written in our own time".

The emphasis is perhaps on the modern writers. Plato, Aristotle, Plotinus and Reynolds make a brief appearance in the first section ("What is Art?"), along with Fry, Maritain, Croce, Cassirer and DeWitt Parker. This section does give a fair representation of the main aesthetic theories, though, as is inevitable in books of this kind, the extracts are rather too short for the student to grasp all the implications of each theory. Some of them, however, are supplemented by the discussions of more specific questions in later sections: thus the student who is baffled by Croce will find more about Expressionism in the extract from Collingwood and the criticism of it by Hospers, and some relevant material in the discussions, by Eduard Hanslick and J. W. N. Sullivan, of whether and in what sense music has "meaning".

The editor himself has an article in which he argues that "art" is a word (like "game" according to Wittgenstein) with a "family" of meanings. (This article is accompanied by a reply to it, specially written for this book, by Erich Kahler.) It is perhaps because he holds this view that Weitz has included a large number of extracts from works on this or that particular art by writers who are not concerned with general aesthetic theory. Thus we have Matisse and Bernhard Berenson on painting, Geoffrey Scott and J. M. Richards on architecture, Henry Moore on sculpture and J. J. Martin on dancing, a lengthy extract from Rosenthal and Smith's *Exploring Poetry,* and much else of the same general kind. This is in line with Beardsley's approach; and it is certainly important that students of aesthetics should be acquainted with this kind of material as well as with the more abstract speculations of the philosophers.

For the rest, there is Freud on sublimation (rather too briefly), Nietzsche on the distinction between the Dionysiac and the Apollonian elements in art, Hume's essay on tragedy (along with A. C. Bradley and J. W. Krutch on the same subject), and Bullough on "psychical distance", to mention some of the better known contributions to the subject. There is a good deal of less familiar material as well. Altogether, this should make a valuable source-book. And, between them, these two books should form the basis of an admirable course on aesthetics.

D. H. MONRO.

THE INDIAN JOURNAL OF PHILOSOPHY, Vol. 1, No. 1, August, 1959. Edited by J. D. Swamidasan, with the co-operation of F. Cioffi, S. Divatia, E. Kamenka, R. F. Khan and K. J. Shah. Published on behalf of The Association for Philosophical Research, Bombay, by The Popular Book Depot, Lamington Road, Bombay 7. This number 72 p.; 3 issues a year, August—December—April. Annual subscription Rs. 5 (10s. sterling).

This new journal is a most welcome one. It promises to bring India right into the main stream of world philosophy. No one with any acquaintance with ancient and mediaeval Indian systems of philosophy can fail to admire these great intellectual monuments, but unfortunately this very excellence of past achievements has led Indian philosophers too much to look backwards towards their own philosophical heroes of ancient and mediaeval times. The greatest of these was perhaps Sankara, who is strongly reminiscent of F. H. Bradley, and so it is not surprising that when Indian philosophers have turned to the west it has been to the now, I hope, moribund idealist tradition. This, I think, explains why Indians have not made contributions to modern philosophy comparable to those which, for example, they have made to mathematics and physics. Now, at last, with the publication of this new journal, Indian philosophers look like emerging from this unfortunate isolation.

The first article in this issue is by K. R. Popper, on "The Sources of Our Knowledge". Those who, like me, greatly admire many of Popper's writings, will be disappointed that this is a mere five pages, rather diffusely written. Some of it seems platitudinous, as when he answers the question "How can we hope to detect error?" by saying "By criticising the theories of others and—if we can train ourselves to do so—by criticising our own theories". Part of the trouble is, I think, that the article was not specially written as such but is an extract from a forthcoming book, and so is torn from its context. There follows a baffling article on "Some Reflections on Meaning", by J. N. Findlay. After the heady wine of Popper and the obscurity of Findlay (how he has changed from the Findlay of twenty years ago!) I turned with some relief to the elegantly and straightforwardly written article on "Justification in Ethics" by Rajendra Prasad. The remaining article, on "Karl Marx's Analysis of Law", is by Alice Tay Erh Soon and E. Kamenka. I do not feel competent to comment on it, though I wonder whether its place should not be in a journal devoted to political theory. There is an interesting critical notice by Passmore of Miss Anscombe's *Intention.*

Readers of the *A.J.P.* may be interested in the slight Australasian flavour of this number of the journal. Popper and Findlay have taught in New Zealand, Kamenka is from Sydney, Passmore we all know, one of the two book reviews is of Baier's book on ethics, and the other is of a compendium one of whose editors is a Melbourne graduate. The *Indian Journal of Philosophy* is like the *A.J.P.* in not being as thick as we would wish it to be. Perhaps the reason is one of finance, as unfortunately it is in our case. The journal is well printed and quite well produced.

J. J. C. SMART.

# BOOKS RECEIVED

(Mention in this list neither precludes nor guarantees later review.)

**Benn**, S. I., and **Peters**, R. S. Social principles and the democratic state. London, Allen & Unwin, 1959. 403 p. 32s. (U.K.)

Intended primarily as a text-book for students of social philosophy. Specially concerned with the relations between moral and political theory, and with the analysis of such concepts as "justice", "rights", etc.

**Bergmann**, Gustav. Meaning and existence. Madison, University of Wisconsin Press, 1960. xi, 274 p. $6.50.

Fourteen essays, all previously published, centring round the analysis of mind and the possibility of constructing a formalised language that will do justice to mental events.

**Bergson**, Henri. The philosophy of poetry: the genius of Lucretius; edited, translated and in part recast by Wade Baskin. New York, Philosophical Library, 1959. 83 p. $2.75.

The notes and commentary from the edition of *De Rerum Natura* published by Bergson in 1884.

**Bickel**, Lothar. The unity of body and mind; edited and translated by Walter Bernard. New York, Philosophical Library, 1959. 167 p. $3.75.

Posthumously published work of a German Idealist.

**Da Vinci**, Leonardo. Philosophical diary; translated with an introduction by Wade Baskin. New York, Philosophical Library, 1959. 87 p. $2.75.

Extracts from the notebooks.

**Dewey**, John. Dictionary of education; edited by Ralph B. Winn. New York, Philosophical Library, 1959. x, 150 p. $3.75.

Aphorisms extracted from Dewey's writings and arranged under subject headings in alphabetical order, thereby destroying any connected argument.

**Duncan**, A. R. C. Practical reason and morality; a study of Immanuel Kant's *Foundations for the Metaphysic of Morals*. London, Nelson, 1957. 18s. (U.K.)

Argues that Kant's moral theory has been commonly misinterpreted. Kant did not set out to discover a rule of conduct but to analyse the concept of obligation, in order to throw light on the problem of synthetic a priori propositions.

**Feibleman**, James K. Religious Platonism; the influence of religion on Plato and the influence of Plato on religion. London, Allen & Unwin, 1959, 236 p. 25s. (U.K.)

Argues that Plato had two distinct philosophies: an objective idealism which derives from the pre-Socratics and the Olympic religious beliefs, and a metaphysical realism which derives from the Orphic cult. It is mainly the latter which has influenced Christianity, through Neoplatonism, which is both a selection from, and a distortion of, Plato.

**FREUND**, Hans. The balanced life; an essay in ethics. New York, Philosophical Library, 1959. 186 p. $4.50.

Discusses the good life and how it is to be attained. After some discussion of Epicurus, Mill and Aristotle, the author concludes that the good life is the life of "creativeness" and "fellowship" and is to be attained through education, especially religious education.

**FUJISAWA**, Chikao. Zen and Shinto; the story of Japanese philosophy. New York, Philosophical Library, 1959. 92 p. $2.75.

Not history of philosophy, but Shintoist apologetics. "We Neo-Shintoists hold human existence to be pre-reflectively constitutive of and connatural with, transcendence and immanence, for man is called upon to form a metaphysical trinity together with heaven and earth."

**GARDINER**, Patrick, ed. Theories of history; readings from classical and contemporary sources. Glencoe, Free Press; London, Allen & Unwin, 1959. ix, 549 p. $8.50.

Part I contains extracts from such writers as Vico, Kant, Hegel, Comte, Marx, Spengler, Toynbee, Croce and Collingwood; Part II contains some criticism of these classical theories (from Popper, Walsh, Russell, Berlin, Geyl and Blake), and some recent writing on historical explanation (Hempel, Nagel, Gallie, Dray, White, Frankel, Donagan, Scriven) and its place in the social sciences (Mandelbaum, Gellner, Watkins).

**HART**, H. L. A., and **HONORE**, A. M. Causation in the law. Oxford, Clarendon Press, 1959. xxxii, 454 p. £4/9/3 (Australian).

Recent legal theorists have argued that many so-called causal questions (e.g., whether plaintiff's injury was the consequence of defendant's action) are really policy questions in disguise. This book analyses typical cases in order to show that this contention is only partly true. Of great interest to philosophers as well as to lawyers.

**HEGEL**, G. W. F. Encyclopedia of philosophy; translated and annotated by G. F. Mueller. New York, Philosophical Library, 1959. 287 p. $6.

"I have dared to translate, not the ponderous Hegelian jargon, which is as little German as it would be English, but the thought. My 'translation', then, is a critical presentation or rendition."

**JUNG**, C. G. Collected works, volume 9, part I: The archetypes and the collective unconscious. London, Routledge & Kegan Paul, 1959. xi, 462 p. 52s. 6d. (U.K.)

**KANT**, I. Foundations of the metaphysics of morals, and What is enlightenment? Translated, with an introduction, by Lewis White Beck. New York, Liberal Arts Press, 1959. 80c. (Paper covers.)

**KROOK**, Dorothea. Three traditions of moral thought. Cambridge University Press, 1959. xiii, 355 p. 30s. (U.K.)

One tradition is represented by Plato and St. Paul, another by Aristotle, Hobbes and Hume. These are the Scylla and Charybdis between which the third triumphantly steers. It is represented by Mill, Matthew Arnold, Bradley and D. H. Lawrence. What these odd bed-fellows have in common is "a religious Humanism", which finds its supreme good, not in other-worldliness on the one hand nor in social convenience on the other, but in selfless love for other human beings.

**LAMONT**, Corliss, ed. Dialogue on John Dewey. New York, Horizon Press, 1959. 155 p. $2.50.

**LAMONT**, Corliss, ed. Dialogue on George Santayana. New York, Horizon Press, 1959. 115 p. $2.50.

Each of these books is "the transcription of an informal evening of reminiscences and personal impressions" of Santayana or Dewey. Seven of Santayana's friends, and eleven of Dewey's, took part.

**LARSON**, Martin A. The religion of the Occident; or, The origin and development of the Essene-Christian faith. New York, Philosophical Library, 1959. xx, 711 p. $6.00.

"The teachings of Jesus are traced to their immediate and ultimate sources in Essenism, Judaism, Pythagoreanism, Zoroastrianism, Buddhism, and the mystery cults of Greece, Asia Minor and Egypt." Blurb.

**MARTIN**, C. B. Religious belief. Ithaca, Cornell University Press, 1959. $3.00.

"I shall try to set out as clearly as possible what I believe to be some of the most important difficulties and confusions in religious forms of argument and assertion; then I shall give accounts of what such forms of argument and assertion might mean that would be safe from philosophical censure." Preface. Not a great deal of orthodox religious belief survives this translation.

**MURRAY**, Byron D. Commonwealth of Americans. New York, Philosophical Library, 1959. 219 p. $3.75.

The American mind as revealed by literature and popular theology.

**NIETZSCHE**, F. Unpublished letters; translated and edited by Kurt F. Leidecker. New York, Philosophical Library, 1959. 156 p. $3.75.

**ORTEGA Y GASSET**, José. Man and people. London, Allen & Unwin, 1959. 272 p. 25s. (U.K.)

Sets out to answer the question: "What is society?".

**OSBORN**, R. Humanism and moral theory; a psychological and social inquiry. London, Allen & Unwin, 1959. 116 p. 18s. (U.K.)

A defence of ethical objectivism based on sociological and psychological data.

**PETERS**, R. S. Authority, responsibility and education. London, Allen & Unwin, 1959. 137 p. 12s. 6d. (U.K.)

Based on talks given over the air and public lectures to teachers. Discusses such problems as the place of religion in education, and the bearing of Freud and Marx on educational theory.

**PLATO**. Gorgias; a revised text with introduction and commentary by E. R. Dodds. Oxford, Clarendon Press, 1959. 406 p. £3/13/0 (Aust.).

**REXINE**, John E. Religion in Plato and Cicero. New York, Philosophical Library, 1959. 72 p. $2.75.

**RUNES**, Dagobert D. Pictorial history of philosophy. New York, Philosophical Library, 1959. x, 406 p. $15.00.

The implicit definition of philosophy is wide, and the text defective in various ways, but the pictures (mainly portraits) are excellent.

**STIERNOTTE**, Alfred P. Ed. Mysticism and the modern mind. New York, Liberal Arts Press, 1959. xiv, 206 p. $4.50.

Eleven essays, by philosophers and theologians, on such topics as the mystical element in Whitehead, in Emerson, in Wittgenstein, in existentialism, and in Indian thought.

**STRAUSS**, Leo. What is political philosophy? and other studies. Glencoe, Ill., Free Press, 1959. 315 p. $6.00.

Ten articles, all previously published. Includes essays on Hobbes, Locke, Plato and Maimonides, as well as some on more general topics.

**WAITE**, A. E. The open vision: a selection from the poems of Arthur Edward Waite. Windsor, Shakespeare Head Press, 1959. xxi, 92 p. 15s. (U.K.)

**WATERMAN**, Leroy. Forerunners of Jesus. New York, Philosophical Library, 1959. 156 p. Price not given.